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**THE EFFECT OF MINDFULNESS-BASED STRESS REDUCTION  
ON QUALITY OF LIFE  
A meta-analysis**

Jane V. Russell

Doctorate in Clinical Psychology  
The University of Edinburgh  
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## Declaration of Own Work

I confirm that all this work is my own except where indicated, and that I have:

- Composed and undertaken the work myself ☒
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- Given the sources of all pictures, data etc. that are not my own ☒
- Not made undue use of essay(s) of any other student(s) either past or present (or where used, this has been referenced appropriately) ☒
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- Acknowledged in appropriate places any help that I have received from others (e.g. fellow students, technicians, statisticians, external sources) ☒
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## 1. OVERVIEW OF THESIS

This thesis consists of two sections, the first of which is a systematic review of self-report measures of mindfulness. The review aims were to evaluate the psychometric properties of each of the identified measures and examine their utility for research and clinical practice. Definitions of mindfulness were central to the differences found between measures, and as such this review also provides an overview of how mindfulness has been conceptualised in the literature. This review has been presented in the format required by the journal, *Clinical Psychology Review*.

The second section is a meta-analysis which examines the efficacy of mindfulness-based stress reduction (MBSR). Mindfulness-based interventions are increasingly being applied in a range of settings and the evidence base is growing. Specifically, this review aimed to determine the effectiveness of MBSR on quality of life for people suffering from chronic physical health conditions. The methods and results of the meta-analysis are described in detail, followed by a discussion of the findings. A more concise overview is then provided as a journal article, in the format required by the *British Journal of Clinical Psychology*. The guidelines for submission to both journals are included in Appendix 1 and 2 respectively.

## 2. SYSTEMATIC REVIEW

### A SYSTEMATIC REVIEW OF SELF-REPORT MEASURES OF MINDFULNESS

Authors: Jane V. Russell<sup>ab</sup>, Dr Paul Graham Morris<sup>a</sup> and Dr Andrew J. A. Keen<sup>b</sup>

<sup>a</sup> Clinical and Health Psychology, University of Edinburgh, UK

<sup>b</sup> NHS Grampian

Address correspondence to:  
Jane Russell  
Clinical and Health Psychology  
School of Health in Social Science  
University of Edinburgh  
Edinburgh  
EH8 9AG

Email: [janevrussell@gmail.com](mailto:janevrussell@gmail.com)

Word Count: 11,562



## **ABSTRACT**

There is substantial evidence that mindfulness-based therapies can reduce symptoms and improve well-being for individuals with physical and mental health problems. In order to evaluate these interventions and understand why they are effective it is necessary to have methods to measure the construct, mindfulness. The aim of this review was to identify available measures of mindfulness and consider their psychometric properties, interpretability and utility for research and clinical practice. A systematic search of the literature identified ten measures, all of which are in the relatively early stages of development. The measures were based on several different conceptualisations of mindfulness and therefore a significant degree of variation between measures was present. The psychometric properties of each measure were summarised and directions for future research considered.

*Keywords:* Mindfulness, self-report measure, questionnaire, reliability, validity, psychometric properties, systematic review

## **2.1. INTRODUCTION**

The application of mindfulness-based therapies in clinical settings has very much preceded our understanding of what mindfulness is and how it works. Despite this, the evidence base for a range of physical and mental health conditions is growing and shows promising effects (for a review see Hofmann, Sawyer, Witt, & Oh, 2010). With this increase has come a need for an adequate definition of mindfulness and valid, reliable methods of assessment.

Mindfulness refers to a skill which can be developed through the practice of meditation and involves bringing a particular quality of attention to the present moment (Bishop et al., 2004). Ultimately, it is a state of consciousness which is characterised by an attitude of acceptance towards internal and external experiences (Brown & Ryan, 2003).

Mindfulness training is a component, with varying degrees of emphasis, in interventions such as Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn, 1990), Mindfulness-Based Cognitive Therapy (MBCT; Segal, Williams, & Teasdale, 2002), Dialectical Behaviour Therapy (DBT; Linehan, 1993); and Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999). Following mindfulness-based therapy, improvements have been observed in stress (Carlson, Ursuliak, Goodey, Angen, & Specia, 2001), chronic pain (Morone, Greco, & Weiner, 2008), depression (Teasdale et al., 2000), and quality of life (Grossman et al., 2010). Formal mindfulness meditation practice and improvements in symptoms and well-being appear to be mediated by increases in mindfulness (Carmody & Baer, 2008).

The philosophical root of mindfulness meditation lies in Buddhism. However, as mindfulness-based interventions are increasingly proving to be effective, there has been an emphasis on determining an operational definition in order to explore the mechanisms involved with empirical research. Over the past decade there has been debate about definitions of mindfulness which in turn has affected how it is measured. Two main questions have arisen: is mindfulness best defined as a 'trait' or a 'state' and is it a multidimensional or single-factor construct?

The term trait mindfulness is used in the literature to describe an individual's dispositional level of mindfulness in day to day life (Brown & Ryan, 2003). It is based on the assumption that individuals have a relatively stable propensity to be mindful in everyday life and that this can be increased with more regular practice, for example through formal meditation practice (Kabat-Zinn, 2003). However, it is also acknowledged that an individual, at any one point in time, can be in a particularly mindful or mindless *state* (Brown & Ryan, 2003). Individual differences in state mindfulness, therefore, may refer to exactly *how* mindful one can be at any given moment. The extent to which an individual's capacity to evoke a mindful state is related to their more stable propensity to be mindful in daily life remains to be determined (Baer, Smith & Allen, 2004).

Concerning the multidimensional nature of mindfulness, one of the most widely cited definitions is that of Bishop et al. (2004) who argue for two key components. The first is the self-regulation of attention to increase awareness of present moment experiences. This refers to the ability to observe thoughts, feelings, physical sensations and other experiences as they occur moment by moment. The distinction here is for the mind to be focussed on what is going in the here and now, rather than being overly caught up in or distracted by thoughts and images relating to the past or future (McCracken, 2011). The second component is adopting an attitude to those experiences which is open, curious and accepting. This is particularly important for negative experiences, where the temptation may be to automatically judge, ignore or minimise that experience (Coffey, Hartman, & Fredrickson, 2010).

From a DBT perspective however, mindfulness is conceptualized as a group of six discrete but related skills referring to what one does when being mindful and how one does it. Consequently, this definition includes additional elements, such as the ability to label one's experiences with words (Linehan, 1993). Shapiro and colleagues propose a definition which is similar to that of Bishop, but stress the role of 'intention'. The perceived reason for *why* one is practicing meditation has been linked to outcomes in earlier research (Shapiro, Carlson, Astin, & Freedman, 2006). To complicate matters further, whilst mindfulness is generally described in the literature

as multifaceted, several existing measures of mindfulness have not supported this theory (e.g. Buchheld, Grossman, & Walach, 2001; Feldman, Hayes, Kumar, Greeson, & Laurenceau, 2007).

Various hypotheses about why mindfulness may be beneficial are summarised by Baer (2003). For example, it has been argued that mindfulness involves exposure to distressing thoughts and feelings and therefore improves a person's ability to tolerate their experiences without becoming emotionally overwhelmed (Kabat-Zinn et al., 1992). Others argue that mindfulness invokes cognitive changes which help an individual to distance themselves from their thoughts and view them as mental events rather than "direct readouts on reality" (Teasdale, 1999, p.146). This is often referred to in the literature on psychotherapy as the "observing mind" and is often part of the goal of many approaches, including CBT. It has also been suggested that mindfulness is a way of increasing the ability to be accepting towards negative thoughts and feelings which, in turn, can alter their impact (Hayes, Wilson, Gifford, Follette, & Strosahl, 1996).

The mechanism by which these hypothesised changes occur is currently unclear but this may be due, for example, to desensitisation following repeated exposure to potentially distressing thoughts and images. Alternatively people may relate to their internal experiences differently with increased ability to simply "observe" them. Relaxation, although not the goal of mindfulness meditation, is thought to be a bi-product which may also contribute to positive outcomes. However, recent evidence suggests that compared to relaxation MBSR is associated with greater levels of parasympathetic activation, which may be beneficial for reducing pain (Ditto, Eclache, & Goldman, 2006). MBSR also appears to be more effective than relaxation in terms of enhancing positive states of mind (Jain et al., 2007).

Attempts to measure mindfulness have predominantly involved the use of self-report questionnaires. However, neurological and neuropsychological methods are increasingly being employed, a development which has significantly improved our understanding of mindfulness. A review of neuroimaging studies indicates that

meditation clearly affects the central nervous system, demonstrated by greater activation in the frontal and prefrontal areas (Cahn & Polich, 2006). Farb and colleagues investigated the neural expression of sadness following mindfulness training and found significant differences in brain activity compared to a control group (Farb et al., 2010). However, the long term effects of meditation on brain activity are yet to be established. Chambers, Lo and Allen (2007) investigated the impact of meditation training on working memory, sustained attention and attention switching, and found significant improvements relative to a control group. The ability to regulate attention is central to the operational definition of mindfulness and increasingly this is being investigated as a way to explore the mechanisms of change (Chiesa, Calati, & Serretti, 2010).

There are obvious advantages to measuring and exploring mindfulness with the methods described above. Self-report questionnaires depend on subjective ratings which tend to fall prey to biases such as acquiescence (yes-saying) or end-aversion bias (the reluctance of some people to use extreme categories of a scale). There are also the cognitive requirements of understanding the question, estimating a response based on the frequency of a behaviour or state of mind and then mapping that answer onto a given scale (Streiner & Norman, 2003).

However, despite the advances in alternative methods of assessment, the development of self-report measures of mindfulness is important for a number of reasons. In order to evaluate and improve interventions such as MBSR and MBCT, it is necessary to establish to what extent they increase an individual's ability to be mindful. At present, self report measures are the only feasible and meaningful way to do this. In addition, over recent years, clinicians and researchers have become increasingly interested in not only whether or not an intervention works, but also in what ways. Being able to disentangle interventions, therefore, allows researchers to conduct statistical analysis to explore factors that mediate or moderate patients' outcomes. Finally, the development of self-report measures requires clear definitions and therefore empirical research of these measures can provide important

information about the construct of mindfulness itself (Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006).

In summary, measurement is a key component in the development of empirical evidence on the utility of mindfulness and mechanisms of change. There is a lack of literature describing the relative strengths and weaknesses of the burgeoning number of scales that propose to measure mindfulness. The objective of this systematic review therefore was to identify self-report measures of mindfulness and critically evaluate their psychometric properties, interpretability, and utility for research and clinical practice.

## **2.2. METHODS**

### **2.2.1. Selection of studies**

Measures were identified by searching the literature for published and unpublished studies which examined the psychometric properties, interpretability and feasibility of self-report measures of mindfulness. Only studies published in English, and investigating a mindfulness measure written in English, were included in this review. Descriptive studies such as review articles and commentaries were excluded as the aim was to identify published and unpublished measures of mindfulness only.

### **2.2.2. Types of measures**

Both ‘state’ and ‘trait’ self-report questionnaires which claimed to measure mindfulness in an adult population were included in this review. The literature on mindfulness in children, perhaps more so than other areas, is particularly limited. Although there are several measures which claim to assess mindfulness in this population (see Appendix 4), the construct may be less clearly defined (Greco et al., 2011) and it was considered out with the scope of this review to consider these issues in detail.

It was necessary to define criteria for measures of related constructs where the authors do not explicitly claim to measure mindfulness. Although mindfulness has been conceptualised in many different ways, the most widely cited and accepted

definition of Bishop et al. (2004) was used to exclude or include studies (Keng et al. 2011). Bishop and colleagues propose that mindfulness consists of two key elements: a) the self-regulation of attention (often referred to as observing and acting with awareness) and b) adopting a particular orientation of acceptance towards one's experience (commonly referred to as non-judging and non-reacting). Therefore, when mindfulness was not explicitly referred to by the authors of a particular test, it was included if it was a measure of both attention and acceptance, regardless of whether or not other aspects of mindfulness were assessed as well. Measures therefore not included in this review were those developed to assess related but distinct concepts such as cognitive fusion, rumination and meta-cognitive awareness.

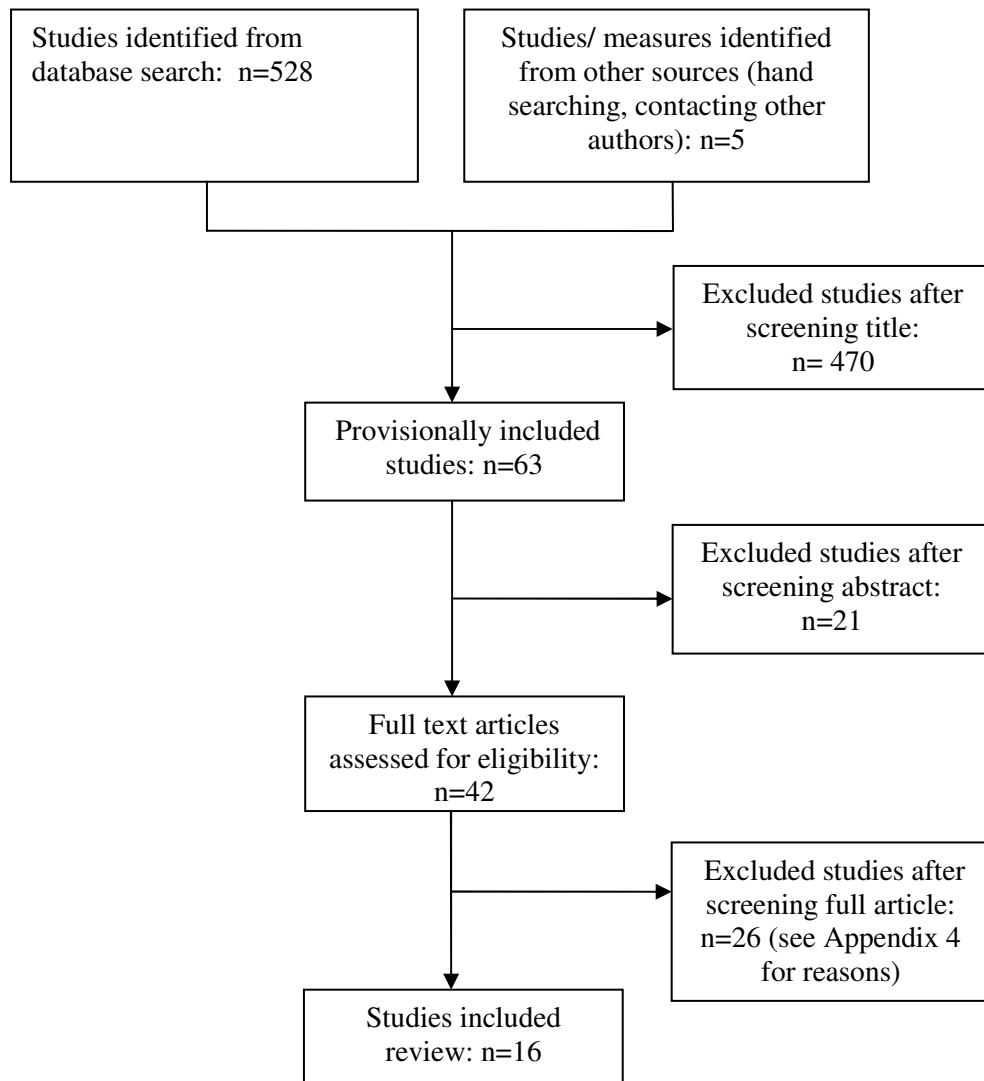
### **2.2.3. Literature search**

The main search to identify relevant studies was conducted in May 2011. Initially, The Cochrane Library was searched to identify any previous systematic reviews of mindfulness measures of which there were none. The following databases were then searched using EBSCOhost: Medline, CINAHL, PsycInfo, and Psychology and Behavioral Sciences Collection (1916-May week 4, 2011). The search terms included were: 'mindful\*' OR 'meditat\*' anywhere in the text, and 'questionnaire', 'measure\*', 'instrument' OR 'scale' in the subject heading<sup>1</sup>.

Specific names of mindfulness measures were also used as search terms once these measures had been identified. Details of the search are included in Appendix 3. Search results were filtered to include only human and English language studies. Reference lists from all included studies were screened by hand to search for further papers. The first authors of all identified studies were contacted to enquire about whether or not they knew of any additional measures of mindfulness. A flowchart illustrating the search process is shown in Figure 2.1.

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<sup>1</sup>An asterisk (\*) results in a search of all available forms of that word. For example, mindful\* will find the words mindfulness, mindfully or mindful.



**Figure 2.1.** Flow chart illustrating literature search process

#### **2.2.4. Methods of reviewing studies**

A systematic approach to evaluating the identified measures was taken by using criteria specified prior to the search (Table 2.1). It was considered important to specify criteria in advance in order to limit the subjective bias that might arise from reviewing the quality of measures informally, and to allow for the relative strengths and weaknesses of the identified measures to be clearly interpretable.

As far as the author is aware, there is no published quality criteria for reviewing mindfulness measures specifically. However, a review of the literature did identify



published criteria for evaluating measures of quality of life. These were developed by the Scientific Advisory Committee (SAC) of the Medical Outcomes Trust (2002) in recognition of the rapidly increasing number of quality of life measures which vary significantly in their method of development, content, breadth of use, and quality. The SAC (2002) specified the key attributes which should be used to determine the quality of self-report measures. These criteria were further discussed and refined by Terwee et al. (2007) who extended these attributes and propose a set of explicit criteria on which questionnaires could be rated. Terwee and colleagues acknowledge that ultimately these criteria are "opinion based" as there is currently no empirical evidence to support quality criteria for self-report measures.

Although an extended version of these criteria, the COSMIN checklist (Mokkink et al. 2010), has been proposed more recently, the 98 items of this scale were considered to be too detailed for the purposes of this review. The criteria originally proposed by Terwee et al. (2007) were therefore considered to be adequate based on their widespread use in other peer-reviewed studies (e.g. Windle et al. 2011, Reneman et al., 2010, Brohan et al., 2010); the fact these criteria are founded on the research by the SAC; and they are in line with guidance given by Streiner and Norman (2003). These criteria are by no means an exhaustive list. Important factors in the quality of a measures such as the size and diversity of sample used to determine psychometric properties, and the number of hypotheses confirmed to justify validity are not included. However, where appropriate these issues are considered in the process of reviewing measures, albeit informally.

One of Terwee's criterion, responsiveness, defined as a questionnaire's potential to detect clinically important changes over time, was excluded from the criteria for this review. This was due to the varying methodologies used in the primary studies to measure responsiveness to change and the highly specific nature of Terwee's criteria for this to be met. Responsiveness of the measures to detect clinically important changes over time was considered qualitatively however in the process of reviewing studies.

Measures were rated on seven individual criteria using the following descriptive terms: ‘well addressed’; ‘adequately addressed’; or ‘poorly addressed’, ‘not addressed’, ‘not applicable’ or ‘information not available’. In addition to these, data extraction focussed on the definition of mindfulness assumed by the authors, a description of the measure, and how the measure was initially developed. Item length, feasibility, and diversity of the sample in which the measure was tested were considered, alongside obvious strengths and limitations of each measure for the purposes of research and clinical practice.

**Table 2.1.** Criteria used for assessing measures

Category	Description	Criteria
Content validity <sup>1</sup>	The extent to which an instrument samples all of the relevant or important domains	Measurement aim is clearly defined, and a clear description of item selection is provided
Internal consistency	The degree to which individual items correlate with scores on all other items	Factor analysis was applied and Cronbach’s alpha scores are between 0.7 and 0.95 for either subscales or total score
Criterion validity	This reflects the relationship between the measure and existing measures of the same construct	Compared to a “gold standard” assessment, correlation is at least 0.7
Construct validity	This refers to whether a scale is correlated with constructs to which the theory predicts it should be	Hypotheses are specified in advance and at least 75% of results are in correspondence with these hypotheses, in subgroups of at least 50 participants
Reliability	A measure of consistency and reproducibility of a test	Test-retest reliability was examined in a sample size of at least 50 participants, with a weighted Kappa of at least 0.7
Floor and ceiling effects	The number of respondents who achieved the lowest or highest possible score	No floor or ceiling effects are present in a sample of at least 50 participants
Interpretability	The degree to which one can assign qualitative meaning to quantitative scores	Means and standard deviations are presented for a general population stratified by age/gender, or at least 4 subgroups

Adapted from Terwee et al. (2007)

<sup>1</sup> Whilst this was partially addressed by defining mindfulness in the inclusion criteria, it was considered important to include this in the ‘formal’ assessment of overall psychometric quality.

## **2.3. RESULTS**

There are three parts to this section: the first describes results from the literature search; the second is a brief overview of the identified measures; and the third is a detailed description of each measure in turn, highlighting key aspects of its development and psychometric properties.

### **2.3.1. Results of literature search**

Sixteen studies were identified from the search, describing a total of ten different measures of mindfulness which met the inclusion criteria. There were a total of ten different first authors and all replied to a request for any additional measures that they were aware of. Only one measure was suggested which had not already been identified from the search: the Comprehensive Inventory of Mindful Experiences (CHIME, Bergomi, Kupper & Tschacher, 2011). It was established through contact with the author that this measure had not been published in English and thus did not meet the criteria for this review.

A total of 26 articles describing 18 different measures were excluded because they did not meet the inclusion criteria. The reasons for exclusion for these 26 articles are included in Appendix 4. Of the 18 measures excluded, 5 (28%) were excluded because the measure did not assess the attention component of mindfulness; 1 (6%) did not assess acceptance; and 1 (6%) assessed neither aspect of mindfulness. Of the remainder, 4 (22%) were not self-report measures of an individual's level of mindfulness and 2 (11%) were not investigated in an English speaking population. Five (28%) of measures were excluded because they assessed mindfulness in a child or adolescent population.

### **2.3.2. Overview of identified measures**

The measures and their psychometric strengths and weaknesses are summarised in Table 2.2. Of the ten measures identified, all but one was based on the conceptualisation of mindfulness as a trait construct, rather than a state. The measures were either multifaceted (50%), with the number of subscales ranging from two to five, or based on a single-factor construct of mindfulness.

**Table 2.2.** Overview of mindfulness measures included in review

Measure	Source reference	Items	Description and <i>subscales</i>	Content validity	Internal consistency	Criterion validity	Construct validity	Reliability	Floor/ceiling effects	Interpretability
CAMS-R	Feldman et al. (2007)	12	Measures mindfulness as a single factor encompassing four aspects: attention, present focus, awareness, and acceptance of thoughts and feelings in daily experience.	++	++	+	++	-	-	-
FFMQ	Baer et al. (2006)	39	Developed using exploratory factor analysis of 5 other mindfulness measures. Measures mindfulness as a multifaceted construct with 5 subscales: <i>observing, describing, acting with awareness, non-judging of inner experience, and non-reactivity to inner experience.</i>	++	++	+	++	-	-	++
FMI	Walach et al. (2006)	30	Measures non-judgmental observation of and openness to present moment experiences. Designed for individuals with experience of meditation. Single factor construct.	++	++	-	-	-	-	-
KIMS	Baer et al. (2004)	39	Based on DBT conceptualization of mindfulness skills, assesses general tendency to be mindful in daily life. Assesses four facets of mindfulness with the subscales: <i>observing, describing, acting with awareness, and nonjudgmental acceptance.</i>	+	++	+	++	+	+	+
MAAS	Brown & Ryan (2003)	15	Single factor measure of <i>mindlessness</i> (defined as the absence of mindfulness). Measures how inattentive and unaware an individual is of their present moment experience.	++	++	+	++	++	+	+
MMS	Bodner & Langer (2001)	9	Measures mindfulness as a single factor conceptualised as four aspects: novelty seeking, engagement, novelty producing and flexibility.	+	++	-	-	-	-	-

Measure	Source reference	Items	Description and <i>subscales</i>	Content validity	Internal consistency	Criterion validity	Construct validity	Reliability	Floor/ceiling effects	Interpretability
PHLMS	Cardaciotto et al. (2008)	20	Measures two factors of mindfulness with subscales: <i>awareness</i> of internal and external experiences; and having an <i>accepting and non-judgemental</i> stance towards experiences.	+	++	-	+	-	+	+
SMQ	Chadwick et al. (2008)	16	Designed to assess four aspects of mindfulness: mindful observation, non-aversion, non-judgment, and letting go. Aimed at measuring how mindfully individuals respond to distressing thoughts and images. Single factor structure.	-	++	+	+	-	-	-
TMS-State	Lau et al. (2006)	13	Aims to assess state mindfulness and before completion participant is required to practice 15-minute meditation exercise. Measures mindfulness as a two factor construct with subscales: <i>curiosity</i> , which assesses curiosity about inner experiences; and <i>decentering</i> , which relates to not becoming overly involved in inner experiences.	++	++	-	+	-	-	-
TMS-Trait	Davis et al. (2009)	13	Assesses trait mindfulness using same questionnaire as above but with subtle changes in wording and instructions.	++	++	+	-	-	-	-

Measures: CAMS-R: Cognitive and Affective Mindfulness Scale-Revised; FFMQ: Five Facet Mindfulness Questionnaire; FMI: Freiburg Mindfulness Inventory; KIMS: Kentucky Inventory of Mindfulness Skills; MAAS: Mindful Attention Awareness Scale; MMS: Mindfulness/Mindlessness Scale; PHLMS: Philadelphia Mindfulness Scale; SMQ: Southampton Mindfulness Questionnaire; TMS: Toronto Mindfulness Scale

Further abbreviations: ‘++’ = well addressed; ‘+’ = adequately addressed; ‘-’ = not addressed/poorly addressed/not applicable/information not available

### **2.3.3. Detailed description of each measure**

The following describes each measure of mindfulness and key aspects of the available psychometric data, as well as advantages and disadvantages for use in clinical and research settings.

#### ***2.3.3.1. Cognitive and Affective Mindfulness Scale–Revised (CAMS-R)***

##### *Description*

The CAMS-R (Feldman et al., 2007) aims to measure mindfulness as a single factor encompassing the ability to a) regulate attention, b) maintain focus on the present moment, c) have an awareness of that experience, and d) have an attitude of acceptance towards thoughts and feelings.

##### *Reliability and validity*

Participants were 548 university students (mean age=19.31, SD=2.66; Feldman et al., 2007). Internal consistency was demonstrated with Cronbach's alpha coefficients ranging from 0.74 to 0.77. This has been replicated by independent researchers in a different student sample (N=613,  $\alpha=0.81$ ; Baer et al., 2006) and a community sample with chronic health problems ( $\alpha=0.85$ ; Mcphail et al., 2005).

Criterion and discriminant validity was assessed using the Freiburg Mindfulness Inventory (FMI; Buchheld et al., 2001) and Mindfulness Attention and Awareness Scale (MAAS, Brown & Ryan, 2003) and correlations were  $r=0.51$  and  $r=0.66$  respectively. In addition, scores on the CAMS-R were found to be positively correlated with well-being ( $r=0.47$ ). Discriminant validity was demonstrated with depression ( $r=-0.3$ ), anxiety ( $r=-0.24$ ), worry ( $r=-0.46$ ) and thought suppression ( $r=-0.47$ ).

Interestingly, Baer et al. (2006) found that the CAMS-R was not significantly correlated with meditation experience ( $r=0.2$ ,  $p=0.06$ ). However, their sample contained a disproportionate number of people with little or no meditation experience (72%) and it is possible that the variation in meditation experience was not sufficient to suitably test this relationship.

### *Strengths and limitations*

A strength of this measure is that it is brief and does not require prior knowledge or experience of meditation. Furthermore, there is evidence that it measures something akin to other commonly used mindfulness inventories. However, as there are no subscales which can be scored independently, this scale is limited for research purposes investigating in more detail the mechanisms of change in mindfulness-based interventions. Feldman et al. (2007) used an entirely student sample and data for the CAMS-R is lacking to confirm the structure and psychometric properties in clinical populations.

### **2.3.3.2. Five Facet Mindfulness Questionnaire (FFMQ)**

#### *Description*

The FFMQ (Baer et al., 2006) was developed using items from five existing measures of mindfulness: MAAS, FMI, KIMS, CAMS, and the Southampton Mindfulness Questionnaire (SMQ; Chadwick et al., 2008) to explore the multifaceted nature of mindfulness. Factor analysis revealed five individual facets: *non-reactivity* (e.g. "I perceive my feelings and emotions without having to react to them"), *observing* (e.g. "I pay attention to whether my muscles are tense or relaxed"), *acting with awareness* (e.g. "I rush through activities without being really attentive to them"), *describing* (e.g. "I'm good at finding the words to describe my feelings") and *non-judging* (e.g. "I tell myself that I shouldn't be feeling the way I'm feeling"). All of the items on the *acting with awareness* and *non-judging* subscale are reverse scored.

#### *Reliability and validity*

The FFMQ has been validated with students, and a community sample of adults including both experienced and inexperienced meditators. Cronbach's alpha coefficients for all samples were adequate to good (alphas ranged from 0.67 to 0.92 for individual subscales; Baer, Samuel, & Lykins, 2011; Baer et al., 2008). Correlations between scores on the five FFMQ subscales, psychological well-being, and symptoms were in the expected direction and significant in most cases. Correlations with at least one subscale ranged from  $r=0.42$  to  $r=0.68$ . Scores on the

FFMQ were not compared to other measures of mindfulness and therefore criterion validity was not investigated *per se*. However, as the FFMQ consists of items from other measures which were all significantly correlated with one another (correlations ranged from  $r=0.31$  to  $r=0.67$ ) and so this was considered to be adequately addressed.

Previous research indicated that the FFMQ was unable to distinguish between meditators and non-meditators (Van Dam, Earleywine, & Danoff-Burg, 2009) and as a result the validity of the FFMQ as a pre-post measure has been questioned (Van Dam et al., 2009). However, a weakness of this study was that meditators and non-meditators were not matched demographically, and more recent research indicates these difference are not present when demographics are taken into account (Baer et al., 2011). It is still unclear though whether this finding can be replicated in a sample more representative of the general population, as the sample used by Baer and colleagues' had a high proportion of highly educated health professionals.

#### *Strengths and limitations*

This measure has good utility for research purposes, as it allows for the investigation of specific aspects of mindfulness using subscale scores. So, for example, different facets of mindfulness can be explored for their potential roles as moderators or mediators of patient outcomes. Furthermore, basing its design on the psychometric analysis of items from multiple pre-existing mindfulness questionnaires is systematic and seems sensible. It is however one of the longer measures and is therefore weaker in terms of clinical utility. That is, clinicians looking for a brief, "good-enough", measure of mindfulness may be disinclined to opt for this specific inventory. Normative data is based solely on student and community samples and therefore its potential usefulness with clinical populations is currently unclear.

#### **2.3.3.3. Freiburg Mindfulness Inventory (FMI)**

##### *Description*

The FMI aims to assess non-judgemental, present moment observation and openness to negative experience in individuals with experience of meditation (Buchheld et al., 2001). A short form with 14-items has since been published which the authors



suggest is more suitable for non-meditating samples but this is not available in English (Kohls, Sauer, & Walach, 2009).

#### *Reliability and validity*

The measure was administered to a sample of 196 students to investigate reliability and validity (Leigh, Bowen, & Marlatt, 2005). Construct validity was explored with a measure of spirituality. Factor analysis identified three factors: *acceptance and openness to self and experiences* ( $\alpha=0.76$ ); *mind/body awareness* ( $\alpha=0.73$ ); and *non-attachment to thoughts* ( $\alpha=0.62$ ). Scores on the FMI were significantly correlated with only one of the two measures of spirituality indicating mindfulness and spirituality may be separate constructs. However, the authors did not pre-specify their hypotheses in relation to this. An unexpected positive relationship was found between scores on the FMI and binge drinking. Binge drinkers had significantly higher scores on the FMI than non-drinkers ( $t=-2.78$ ,  $p<0.01$ ).

FMI scores have also been compared to other measures of mindfulness in a different student sample and correlations were significant and as follows: MAAS ( $r=0.31$ ); KIMS ( $r=0.57$ ); CAMS ( $r=0.60$ ); SMQ ( $r=0.45$ ) ( $N=613$ ; Baer et al., 2006).

#### *Strengths and limitations*

This measure is still very much in the early stages of development, particularly the English version, which is an obvious disadvantage. The fact this measure is designed for use with experienced meditators limits its use in both research and clinical settings. The psychometric data published by Leigh and colleagues was with a sample of college students, not experienced in meditation, which limits the interpretability of these results. It is possible that items were misinterpreted by participants and further research on this questionnaire is required in order to clarify its use with non-meditating samples.

#### **2.3.3.4. Kentucky Inventory of Mindfulness Skills (KIMS)**

##### *Description*

The KIMS (Baer et al., 2004) aims to assess mindfulness as a multifaceted construct in the general population and is based on the DBT conceptualisation of mindfulness. Mindfulness is considered to be a set of skills or behaviours including: *observing*, *describing*, *acting with awareness*, and *non-judgemental acceptance*.

##### *Reliability and validity*

Psychometric data comes from a sample of undergraduate students (N=205) and adults diagnosed with borderline personality disorder (N=26) (Baer et al., 2004). Alpha coefficients for the four subscales ranged from 0.83 to 0.91. Test-retest reliability 14 to 17 days after first completion yielded correlations between 0.61 and 0.86. However, the small sample of adults with personality disorder limits the generalisability of this data.

The relationship between the KIMS and other constructs such as personality, psychological symptoms, dissociation and satisfaction with life has been investigated (Baer et al., 2004). Relationships with these constructs were largely in the expected direction, indicating mindfulness, as measured by the KIMS, is associated with greater levels of mental health. More recently, the KIMS has been validated with a clinical sample of 100 adults suffering from recurrent depression (age: M=48.39, SD=11.18; Baum *et al.*, 2010). This study found that the KIMS is sensitive to change following mindfulness interventions such as MBCT. For each of the four subscales, effect sizes ranged from  $d=0.5$  to  $d=0.8$  baseline to post-intervention. KIMS scores have also been compared to other measures of mindfulness and correlations were significant and as follows: MAAS ( $r=0.51$ ); CAMS ( $r=0.67$ ); SMQ ( $r=0.45$ ); FMI ( $r=0.60$ ) (N=613; Baer et al., 2006).

##### *Strengths and limitations*

A strength of the KIMS is that it has been validated with a clinical sample, and has undergone sensitivity to change analysis which proves its utility for both research and in clinical settings where the aim is to evaluate interventions. Of all the

mindfulness measures identified by this review, the KIMS has the strongest correlation with meditation experience which is evidence of its construct validity (Baer et al., 2006). It is however more time consuming to complete than other measures, consisting of 39 items.

### **2.3.3.5 Mindful Attention Awareness Scale (MAAS)**

#### *Description*

The MAAS (Brown & Ryan, 2003) measures an individual's dispositional level of mindlessness (defined as the absence of mindfulness). The authors of this questionnaire assume a definition of mindfulness which is informed by the research and literature on consciousness. Mindfulness, they propose, is simply a form of consciousness which involves a heightened attention to and awareness of the present moment.

#### *Reliability and validity*

The MAAS has been validated in several different studies with students, community samples, and cancer patients (Brown & Ryan, 2003; Carlson & Brown, 2005; Mackillop & Anderson, 2007; Van Dam *et al.*, 2010). Internal consistency was adequate with  $\alpha > 0.7$  in all samples reviewed. Concurrent validity has been assessed with emotional intelligence, well-being, personality, self-consciousness and self monitoring and although correlations were modest, they were in the predicted direction in most cases. Correlation with alternative measures of mindfulness ranged from 0.31 with the FMI (Baer et al., 2006) to 0.7 with the MMS (Bodner & Langer, 2001).

There is some contradicting evidence as to whether the MAAS can reflect differences in meditating and non-meditating samples. Brown and Ryan (2003) found a moderate effect on MAAS scores between these groups ( $d=0.5$ ) whereas Mackillop and Anderson (2007) found no significant differences. This is likely to be a reflection of the type of meditation experience in each of these groups, with the former using participants registered at a community Zen centre, and the latter, a population of students who simply rated whether or not they had experience of

meditation. The MAAS was unable to detect significant changes in a group of cancer patients following a MBSR intervention (Brown & Ryan, 2003).

#### *Strengths and limitations*

The MAAS has undergone extensive psychometric testing by several different research groups and the reliability and validity of this measure is strong. It is one of the few measures to have investigated test-retest reliability and therefore its use as a clinical tool is well-supported. However, to what extent the MAAS reflects differences following a mindfulness-based intervention, and differences between meditators and non-meditators is unclear from the studies reviewed here.

#### **2.3.3.6. Mindfulness/Mindless Scale (MMS)**

##### *Description*

This scale is based on the conceptualisation of mindfulness proposed by Langer (1989) where mindfulness has four components: novelty seeking (the tendency to have an open and curious orientation to one's environment); engagement (an individual's propensity to interact and actively attend to changes in the environment); novelty producing (the propensity to create new meaning and experiences); and flexibility (the ability to view experiences from multiple perspectives and alter behaviour accordingly). The original model proposed by Bodner and Langer (2001) was a 21-item, four factor model to reflect each of these four components. However, more recent research by Haigh and colleagues (2011) did not find support for this and a more reliable nine-item, one-factor self report questionnaire is proposed instead.

##### *Reliability and validity*

The MMS has been validated in a student population (N=582; mean age=19.06, SD=3.5) and Cronbach's alpha for the single-factor measure was 0.79 (Haigh et al., 2011). Construct validity was assessed with affect, emotion regulation and curiosity and correlations were not always in line with predictions questioning this measures construct validity. The MMS was compared to the MAAS and a correlation of 0.7 was identified (Brown & Ryan, 2003).

### *Strengths and limitations*

This is one of the briefest measures of mindfulness and the psychometric data relating to it is limited to a student population. In addition, the construct validity of this measure has only been tested using one other measure of mindfulness. Based on Langer's definition of mindfulness, this measure appears to assess something quite different from other mindfulness questionnaires. For example, items like "*I am very creative*" and "*I like to be challenged intellectually*" seem to relate more to personality than to mindfulness as it is defined elsewhere in the literature (e.g. Bishop et al., 2004).

### **2.3.3.7. Philadelphia Mindfulness Scale (PHLMS)**

#### *Description*

The PHLMS (Cardaciotto, Herbert, Forman, Moitra, & Farrow, 2008) measures the components of mindfulness proposed by Bishop et al. (2004), attention and acceptance, as two separate constructs.

#### *Reliability and validity*

Initially, 204 undergraduate students confirmed the factor structure and internal consistency of the measure (age range: 19-47,  $M=21.9$ ,  $SD=3.38$ ) (Cardaciotto et al., 2008). Internal consistency was adequate for both subscales:  $\alpha=0.81$  for *awareness* and  $\alpha=0.85$  for *acceptance*. The range of scores in this sample of students indicates that ceiling effects may have been present for the *awareness* subscale only.

The measure has also been validated with a larger sample of 559 students who completed an alternative measure of mindfulness, and measures of related constructs such as acceptance and rumination. Convergent validity was investigated using the MAAS and small correlations were found ( $r=0.21$  for *awareness* and  $r=0.32$  *acceptance*). PHLMS subscales were correlated with acceptance, thought suppression and rumination and negatively correlated with depression and anxiety.

The measure has also been validated with two different clinical groups, including individuals diagnosed with a psychiatric disorder and a separate group with an eating

disorder, and alpha coefficients ranged from 0.75 to 0.9. As predicted, the clinical sample had significantly lower scores on both subscales than the non-clinical sample. However, sample sizes were small in both groups (N=30 and N=78), suggesting this research may have been underpowered.

#### *Strengths and limitations*

The PHLMS is a relatively short questionnaire and the subscales allow for examination of the two key components of mindfulness proposed by Bishop et al. (2004). The authors suggest that the use of this measure in clinical settings is premature and that further validation with alternative clinical samples is necessary. The extent to which this measure reflects differences in meditation experience, or changes following mindfulness-based interventions has not yet been investigated.

#### **2.3.3.8. Southampton Mindfulness Questionnaire (SMQ)**

##### *Description*

The SMQ (Chadwick et al., 2008) aims to assess an individual's relationship with distressing thoughts and feelings and items reflect a definition of mindfulness encompassing four factors: decentred awareness of cognitions (i.e. the ability to view thoughts as temporary mental events as opposed to a true reflection of the self); mindful observation (i.e. paying attention to distressing thoughts and feelings without avoidance); non-judgemental attitude towards thoughts and feelings; and 'letting go' of distressing thoughts or feelings without reacting.

##### *Reliability and validity*

Psychometric properties were examined using a combined clinical sample with psychosis (N=122, age: M=31, range 17-72) and a non-clinical sample of both meditators (N=83, age: M=47, range 31-72) and non-meditators (N=51, age M=47, range 23-71) (Chadwick et al., 2008). Cronbach's alphas of 0.89 and 0.82 were found for the community and clinical sample respectively, indicating the SMQ has good internal consistency across different population samples.

Criterion validity was assessed using the MAAS and construct validity by investigating relationships with positive and negative affect, and psychotic symptoms. A significant difference on SMQ scores was found between self-identified meditators ( $M=57.4$ ,  $SD=14.3$ ) and non-meditators ( $M=48.5$ ,  $SD=15.2$ ) in the non-clinical sample ( $p<0.001$ ). Additionally the clinical and non-clinical sample scores were significantly different although these were not matched for age or sex. Scores on the SMQ and MAAS were correlated significantly ( $r=0.61$ ) and higher SMQ scores were associated with positive subjective mood ( $r=0.48$ ) in the non-clinical group. This was also the case for the clinical group: SMQ scores were significantly correlated with negative affect ( $r=-0.62$ ); positive affect ( $r=0.27$ ), and psychotic symptoms ( $r=-0.34$ ).

SMQ scores have also been compared to other measures of mindfulness in a different sample of 613 students and correlations were significant and as follows: MAAS:  $r=0.38$ ; FMI:  $r=0.45$ ; KIMS:  $r=0.45$ ; CAMS:  $r=0.55$  (Baer et al., 2006).

#### *Strengths and limitations*

The SMQ does not assess awareness or acceptance of emotions or external moment-to-moment experiences, instead focussing exclusively on an individual's reaction to distressing thoughts. This could be a limitation as it does not measure aspects of mindfulness which are central to most common definitions (e.g. Bishop et al., 2004; Baer et al. 2006). On the other hand, there has been an effort to establish norms both for clinical and non-clinical samples and the fact that this measure included a community rather than a student sample is a definite strength. In addition, clinicians, and in particular those who are inclined toward cognitive behavioural therapy and theories, may be interested by the focus on intrusive negative thoughts which would often be central to their case formulations and therapeutic efforts. Furthermore, it has been established that this measure is sensitive to differences amongst meditators and non-meditators and this is an advantage both in terms of construct validity. The extent to which this measure is sensitive to change following mindfulness-based interventions is currently unclear and this is typically a crucial issue for clinicians and researchers alike.

### 2.3.3.9. *Toronto Mindfulness Scale (TMS)-State*

#### *Description*

The TMS-State (Lau et al., 2006) is the only measure in this review which assesses mindfulness as a state. Prior to completing the questionnaire, respondents are asked to practice a 15 minute mindfulness exercise, which involves sitting quietly and focussing on the breath. The TMS-State has two subscales: *curiosity* (referring to an awareness of and desire to attend to the present moment) and *decentering* (referring to the ability to refrain from identifying personally with thoughts) and is therefore concurrent with the Bishop et al. (2004) definition of mindfulness.

#### *Reliability and validity*

A community sample of experienced (N=232) and inexperienced (N=158) meditators were used to develop the scale (Lau et al., 2006). Internal validity was investigated and alpha coefficients were 0.84 for *curiosity* and 0.88 for *decentering*. Construct validity was assessed using a variety of self-report questionnaires such as dissociation, absorption, self-awareness and psychological mindedness. The two subscales did not correlate with dissociation as the authors had predicted but did correlate with absorption ( $r=0.31$  and  $r=0.41$ ), internal state awareness ( $r=0.41$  and  $r=0.15$ ), and reflective self-awareness ( $r=0.23$  and  $r=0.42$ ) for each of the two subscales respectively.

Experience of mindfulness meditation was associated with significantly higher scores on both subscales of the TMS-State ( $p<0.001$ ). Incremental validity was conducted by examining changes in TMS-State scores following a meditation programme (MBSR) in a sample of participants with stress, depression, chronic pain and diabetes. Scores on the TMS-State pre and post intervention were significantly improved and effect sizes were moderate ( $d=0.42$  for *curiosity* and  $d=0.6$  for *decentering*). Changes in scores on the *decentering* subscale but not the *curiosity* subscale were predictive of better clinical outcomes, in terms of psychological symptoms and stress.



### *Strengths and limitations*

An obvious limitation of this measure is the need for a preceding meditation exercise which limits its use in both research and clinical practice. In practice, the authors recommend assessing mindfulness with the TMS-State pre-, mid-, and post-treatment because the measure may be limited in terms of its generalisability across mindfulness sessions and mindfulness in everyday life (i.e. trait mindfulness).

#### **2.3.3.10. Toronto Mindfulness Scale (TMS)-Trait**

##### *Description*

The trait version of the TMS (Davis, Lau & Cairns, 2009) is virtually identical to the state version, but with subtle changes to the wording of items and instructions. The TMS-S was modified so that respondents are asked instead to rate items reflecting 'day-to-day' experiences of mindfulness, rather than during a preceding 15 minute mindfulness exercise. Respondents are not instructed to complete the meditation exercise prior to completion of this questionnaire.

##### *Reliability and validity*

A sample of 461 respondents (age  $M=36.5$ ,  $SD=14.3$ ) completed the study and consisted of a mixed community and student sample of meditators ( $N=243$ ) and non-meditators ( $N=218$ ) (Davis et al., 2009). Convergent validity was assessed by administering the TMS-Trait along with six other measures of mindfulness (MAAS, FMI, KIMS, CAMS-R, SMQ and FFMQ). All correlations were significant and ranged from  $r=0.1$  (between KIMS *non-judgemental acceptance* subscale and TMS-Trait *curiosity* subscale) and  $r=0.74$  (between FMI and TMS-Trait *decentering* subscale). Alpha coefficients were 0.91 and 0.85 for the *curiosity* and *decentering* subscales respectively.

Scores on the both subscales of the TMS were significantly higher for meditators as compared to non-meditators ( $t=10.68$ ,  $p<0.0005$  for *decentering* and  $t=-7.91$ ,  $p<0.0005$  for *curiosity*). A ceiling effect for respondents with 20 years of meditation practice or more was found, though not for any other sample. However, these participants represented a small proportion of the overall sample ( $N=27$ ) and the use

of snowball sampling to recruit subjects (where existing study subjects identify future subjects by recommending an acquaintance) may have introduced bias.

#### *Strengths and limitations*

Although this measure has been compared to six other mindfulness measures, including subscales, correlations were in the moderate range at most which may be evidence that criterion validity is poor. Furthermore, the construct validity of this measure has not been investigated and no data relating to clinical samples is available. However, the development of the TMS-Trait allows researchers to investigate the relationship between state and trait mindfulness with comparable measures which is an advantage.

## **2.4. DISCUSSION**

### **2.4.1. General discussion of results**

The aim of this systematic review was to identify and critically evaluate measures of mindfulness. Ten measures were identified and the psychometric properties of these were considered using a set of *a priori* criteria. The results indicated that self-report measures of mindfulness are still in the early stages of development and therefore the reliability and validity of these measures is limited. This is not surprising in view of the fact that research in the field of mindfulness is generally in its infancy (Shapiro et al., 2006). The findings are discussed in relation to each of the criteria used to evaluate the measures, and consequently in terms of what these measures tell us about mindfulness as a construct. Directions for future research are also suggested.

All measures apart from the SMQ demonstrated adequate to good content validity. Generally these measures seem to measure what they propose and the process of item development was typically well described. It was not possible to access the unpublished manuscript relating to the SMQ, which presumably would have described this aspect of the inventory. However, there was also a considerable degree of variation between measures with some emphasising particular aspects and others measuring additional components to the two core components of Bishop et al. (2004).

Internal consistency was rated as 'well-addressed' for all identified measures. However, criterion validity was a weakness in most measures, with only six measures investigating correlation with a "gold standard" assessment - in these cases another previously published measure of mindfulness. Streiner and Norman (2003) highlight the circularity in this method of test development. Presuming a new measure is developed because it is assumed to be somehow superior to previous measures, then to compare them is of limited value. Arguably there is no "gold standard" assessment of mindfulness as measures are in the early stages of development, as is our understanding of mindfulness as a construct.

Despite general agreement in the literature that mindfulness is a multifaceted concept, factor analysis for some measures did not support this and thus measure mindfulness as a single-factor (e.g. CAMS-R, FMI, MAAS, MMS and SMQ). This is likely to reflect differences in the conceptualisation of mindfulness used in the development of these measures. The fact that some existing measures of mindfulness have only moderate correlations with one another (ranging from  $r=0.31$  to  $0.67$ ) is further evidence of this (Baer et al., 2006).

As noted by Grossman and Van Dam (2011) this means a person could score highly on 'mindfulness' on one measure and low on another, which brings into question what exactly is being measured and how valid these tools of assessment really are. In terms of selecting a measure for research or practice, perhaps it is important for the clinician to choose a measure which best captures the aspects of mindfulness the intervention is designed to change. For instance, if the goal of the intervention is to reduce reactions to negative thoughts, a measure which includes 'non-reacting' should be used. If on the other hand the main emphasis is on building acceptance towards experiences then a measure which focuses on this aspect should be used instead.

Construct validity was adequately or well-addressed by 7 (70%) of the measures as relationships between mindfulness and other constructs hypothesised to be related to it were in the predicted direction. In many cases, studies confirmed that mindfulness

is correlated with positive aspects of well-being such as subjective happiness, quality of life, life satisfaction, and negatively correlated with depression, anxiety and other psychological symptoms. However, some of these correlations were noticeably small, or not in the expected direction, and not all of the studies made hypotheses clear about the expected relationships prior to testing.

Reliability was poorly addressed by 8 (80%) of the measures, with only two measures investigating test-retest reliability (KIMS and MAAS). This is a significant weakness of the research in this area, particularly as the majority of measures assume mindfulness is a trait construct, and therefore should be relatively stable over time. Likewise, floor and ceiling effects were only explicitly referred to in the development of one measure (TMS-Trait) and evidence of ceiling effects with meditators with over 20 years of experience was present. Other studies did not provide information about this, or only partially addressed this, for example by describing the range of scores obtained, or by describing the normality of the data.

Interpretability was adequately or well-addressed by only 4 (40%) measures, a reflection of the fact student populations dominate much of the research in this area. This is not especially uncommon in the development of psychological measures. There have been some attempts to validate measures for use with clinical populations and the FFMQ, KIMS, MAAS and PHLMS provided data for the widest range of samples in this respect. However, because a measure is only truly reliable for the population with which it has been validated, all measures identified in this review are significantly limited for wider use.

As highlighted previously, the measures varied significantly in terms of how mindfulness was conceptualised. Some measures for example focus exclusively on reactions to mental events (e.g. SMQ) whereas others include reactions to physical sensations (e.g. FFMQ, KIMS). Others emphasise the ability to describe thoughts and feelings with words (KIMS), or to have curiosity towards internal experiences (TMS-State and Trait).

Two measures which explicitly claim to measure mindfulness were based on a definition which differed significantly from that of Bishop et al. (2004) (MAAS and MMS). The MAAS is one of the most commonly used measures of mindfulness in the literature (Baer & Peters, 2011) and assesses the general tendency to be inattentive to, and unaware of, present-moment experiences. It does not, however, assess in any capacity the tendency to be accepting towards that experience. Acceptance items were initially included in the development of this scale. However the relationship between awareness items and scores on measures of well-being were the same with and without the presence of the acceptance items. Brown and Ryan (2004, p.245) conclude therefore that acceptance is not a distinct facet and as an individual construct is "functionally redundant" in mindfulness.

This has not been the finding of other research, and measures such as the KIMS, PHLMS and FFMQ *have* found that acceptance is an important, and distinct, facet of mindfulness. For example, Baer and colleagues (2006) found that non-reactivity and non-judging of inner experiences are distinct from other facets of mindfulness, both of which they suggest are aspects of acceptance. Despite the fact the MAAS does not measure acceptance, it was still used to determine concurrent (criterion) validity for many of the identified measures. Unsurprisingly, relatively low correlations with the MAAS were found (e.g. FMI:  $r=0.31$ ; KIMS:  $r=0.51$ ; CAMS:  $r=0.51$  and SMQ:  $r=0.38$ ; Baer et al., 2006).

Acceptance does appear to play an important role in the positive outcomes associated with increased mindfulness, as highlighted by the research on self-focussed attention. There is strong evidence to suggest that self-focussed attention, something which mindfulness-based interventions aim to foster, is actually associated with greater negative affect (for a review see Mor & Winquist, 2002). Consistent with this, Baer et al. (2006) found that high scores on the *observing* subscale of the FFMQ were correlated with dissociation, psychological symptoms and thought suppression. Likewise Greco and colleagues (2011) found that the ability to observe thoughts and feelings was associated with thought suppression and somatic complaints in children and adolescents. This suggests that the ability to observe internal experiences may not necessarily be associated with positive outcomes.

Perhaps it is not the occurrence of thoughts and feelings which is important but rather *how* people relate to these once they occur. That is, a difference in attitude might accompany self-focused attention and this is what leads to positive outcomes. Baer et al. (2008) found that the relationship between observing internal experiences, and negative psychological symptoms, was only present in non-meditators. For those with experience of meditation, high levels of observing were associated with greater psychological well-being. It seems, therefore that attention to inner experiences and acceptance of that experience may well be distinct facets and that both may be important components of mindfulness.

The importance of attitude towards thoughts and feelings in mindfulness is further highlighted by studies investigating cognitive reactivity in recurrent depression. Cognitive reactivity refers to the degree to which a mild dysphoric state can trigger negative modes of thinking and feeling which can then escalate to a depressive episode (Raes, Dewulf, Van Heeringen, & Williams, 2009). In recurrent depression, there is some evidence to suggest that individuals with heightened levels of cognitive reactivity are at a higher risk of relapse over an 18-month period post treatment (Segal et al., 2006).

Kuyken et al. (2010) investigated the rates of relapse in 123 individuals with recurrent depression following randomisation to either MBCT or maintenance antidepressant medication. Following participation in MBCT, cognitive reactivation was no longer associated with poor outcomes, as it was in the control group. This was despite the fact cognitive reactivity was actually higher in those who had completed MBCT compared to those on medication. So, although cognitive reactivity was still present in the MBCT group after the intervention, the relationship between cognitive reactivity and depressive symptoms was significantly less. The authors suggest it is the response to these negative thoughts, characterised by an attitude of self-compassion, which is altering their impact at follow-up.

There are, of course, some differences between self-compassion and acceptance as constructs - the former including components such as awareness, self-kindness, and

common humanity. However, a non-judging attitude towards the self is central to both constructs. In fact, Van Dam and colleagues (2011) found that self-compassion was a more reliable predictor of positive outcome following a mindfulness-based intervention than mindfulness. Coffey et al. (2010) found that acceptance of one's experience was a stronger predictor of mental health variables than person-centred attention. Evidently, attitude plays an important role in the positive outcomes associated with mindfulness but this is yet to be fully understood. The fact that the MAAS does not measure any aspect of attitude, be it self-compassion, acceptance or non-judgement, could therefore be viewed as a significant weakness of this measure.

As noted by Grossman (2008), common to most definitions of mindfulness is that it involves paying attention to the present moment. All measures reviewed here assessed, in some form, the ability or tendency of an individual to pay attention to their moment-by-moment experience. However, as discussed, the attitude one adopts whilst doing this may be important also. The Bishop definition incorporates both of these aspects of mindfulness and perhaps for this reason has been cited as "the most integrative and theoretically consistent definitions of the construct" (Van Dam, 2011, p.805). Indeed, this definition was used as the foundation for four of the identified measures reviewed here (CAMS-R, PHLMS, TMS-S and TMS-T). The two facets, attention and acceptance, have been supported by empirical research and confirmed with two separate samples (Coffey et al., 2010).

However, this definition has also received some criticism, particularly in relation to being too narrow, missing out important aspects of the construct. For example, Hayes and Shenk (2004) argue that little emphasis is given to the role of observing and describing present moment experience, both of which they view as behavioural aspects of mindfulness. The Bishop definition is purely a cognitive one, described as a "meta-cognitive skill", despite the fact mindfulness interventions (which aim to foster mindfulness) are usually framed within a cognitive and behavioural perspective. Others argue that Bishop et al. have failed to distinguish between awareness and attention (Rapgay & Bystrisky, 2009; Brown & Ryan, 2004).

Other measures assume quite different definitions of mindfulness. For example, in the KIMS, mindfulness is conceptualised purely as a behaviour. In fact, the goal in the development of this measure was to address the question: "What does one do (or refrain from doing) when being mindful?" (Baer et al., 2004, p.193). Four skills were identified by these authors from the mindfulness literature: *observing*, *describing*, *acting with awareness* and *accepting without judgement*. In support of this conceptualisation of mindfulness, in the development of the FFMQ, these four skills were identified as discrete factors using factor analysis of all existing measures of mindfulness. However, an additional fifth factor, *non-reactivity to inner experiences* - the tendency to allow feelings and thoughts to come and go, without getting caught up in them - was also identified.

Clearly there is a large degree of overlap with the Bishop definition. The factors, *accepting without judgement* and *non-reactivity* both may be seen as ways of operationalising the acceptance component as described by Bishop (Baer et al., 2006). As far as the attentional component is concerned, in the KIMS, both the *observing* and *acting with awareness* subscales have several items referring to attention and overlap extensively with this.

The MMS is based on yet another definition of mindfulness, that of Langer (1989). Langer herself acknowledges that the cognitive state of mindfulness as she defines it, which involves having the cognitive flexibility to view a situation or experience from different perspectives, is different from the Buddhist perspective (and therefore most Western definitions) of mindfulness (Carson & Langer, 2006). Langer's conceptualisation of mindfulness proposes four factors (*novelty seeking*, *engagement*, *flexibility*, and *novelty producing*) yet examination of the factor structure of the initial questionnaire did not support these as distinct aspects of mindfulness (Haigh et al., 2011).

In short, there is no agreed definition of mindfulness and it is clear from the measures reviewed here that the term is used to mean slightly different things by different researchers. Despite this, most measures were largely consistent with



Bishop's definition, although some with more emphasis on the ability to observe and describe experiences with words. What seems striking about the studies in this area is that often in the development of a measure, mindfulness is defined by the researchers based on their experience, knowledge, or literature review of the topic. Items are then selected to reflect this definition and the particular 'theory' of mindfulness is investigated in the process of establishing the psychometric properties of the proposed measure. It seems therefore that our understanding of mindfulness is being driven more by the questionnaires themselves, and empirical data, than by a consistent theoretical model (Hayes & Shenk, 2004; Grossman & Van Dam, 2011).

A weakness of the current measures of mindfulness and research relating to them is the use of a high proportion of reverse scored items, particularly those measuring acceptance. The MAAS, for example, is entirely reverse scored, measuring mindlessness as opposed to mindfulness. The PHLMS *acceptance* subscale is composed entirely of reverse scored items, as is the *accept without judgement* subscale of the KIMS. Acceptance is commonly measured indirectly by assessing how 'judging' and 'reactive' an individual is towards their experiences. This relies on the reverse scoring of items such as: "I tend to evaluate whether my perceptions are right or wrong" (KIMS); or "I try to put my problems out of mind" (PHLMS). The assumption here is that low scores on either of these items is an indicator of how *non-judging* and *non-reactive* a person is, and therefore how accepting they are towards their experiences.

There is some evidence to suggest that indirect assessment, and therefore reverse scoring of items, is a reliable way to measure mindfulness. Brown and Ryan (2003) found a correlation of  $r=0.7$  between the original MAAS, which is entirely reverse scored, and an alternative, directly worded, version of the questionnaire. Given these two version of the questionnaire are supposedly measuring the same construct, this correlation is surprisingly low. Furthermore, high scores on the original version of the MAAS were more strongly related to aspects of well-being than the alternative version. Similarly, Baer et al. (2004) excluded directly worded items from one of the subscales on the KIMS because these items did not correlate highly with the total

score. However, Grossman and Van Dam (2011) criticise this approach and summarise their view succinctly by arguing there is only so much that 'mindlessness' can tell us about being mindful.

In addition to measures relying heavily on the indirect assessment of mindfulness, there is the problem of negatively worded items. These are items which use words such as 'not', 'shouldn't', and 'don't'. For example, an item on the FFMQ is: "I tell myself that I shouldn't be thinking the way I'm thinking". Aside from these negatively worded items being potentially confusing for the respondent, there is an issue about what a positive response to them actually means. It is not necessarily the case that reversing the polarity of an item also reverses the meaning (Streiner & Norman, 2003). Reverse scored items are usually included in questionnaires to prevent the bias that might arise from an individual responding without fully attending to the content. However, when a subscale or measure is composed entirely of reverse scored items, the same potential for response bias is present. In any case, Barnette (2000) found that negatively worded items significantly reduce the reliability of a questionnaire and suggests that alternative methods of reducing response bias should be considered.

Significant differences between the scores of meditators and non-meditators have been demonstrated with some of the measures of mindfulness. On the face of it, this is promising - a measure of mindfulness would be of little use if it did not capture to some extent the differences between those who consciously practice being mindful and those who do not. However, the language of many of the measures is arguably better understood by people with experience of meditation and particularly those who have completed a mindfulness-based intervention.

It is possible that phrases such as "automatic pilot" and "notice my thoughts without reacting" are misinterpreted by those without mindfulness training. Differences in scores may therefore be a reflection of understanding (or misunderstanding) rather than true differences in levels of mindfulness. Where measures have investigated the relationship between meditation experience and mindfulness, few have investigated

the type of experience 'meditators' have. There are qualitative differences between types of meditation (Kabat-Zinn, 2003) and it cannot therefore be assumed that higher levels of mindfulness are necessarily the result of meditation experience. For example, Transcendental meditation has a distinctly different view of the 'wandering mind' than mindfulness.

This review identified one measure of state mindfulness (the TMS-State) which assesses how mindful an individual was during a preceding 15-minute breathing exercise. The other measures assess how mindful an individual has been over time frames ranging from a week (e.g. PHLMS) to more generally in day-to-day life (e.g. CAMS-R, FFMQ and KIMS). There is mixed evidence that the ability to evoke a state of mindfulness is associated with higher levels of trait mindfulness.

Thompson and Waltz (2007) investigated the relationship between what they term 'everyday mindfulness' and mindfulness during meditation. They found no relationship between scores on everyday mindfulness measures (MAAS and CAMS-R) and a measure of mindfulness during meditation (TMS-State) in a group of students. A small correlation ( $r=0.19$ ,  $p<0.01$ ) was found between the TMS-State and the *observe* facet of the FFMQ, but none of the other four facets.

State mindfulness has also been investigated by Brown and Ryan (2003) by giving participants a pager. This was used to prompt their completion of a small sample of items adapted from the MAAS at various time points over a two week period. The authors conclude that trait mindfulness *was* found to be a predictor of state mindfulness. However, whether this method is really assessing state mindfulness is questionable. The psychometric properties of the adapted MAAS items were not investigated and so it is not clear if these items represent a valid assessment of state mindfulness. Furthermore, as there was no control group it is impossible to know how practice effects may have influenced the completion of these items, given participants completed them several times a day for two weeks.

It seems that in these studies state mindfulness is really referring to two quite different things. The TMS-State measures how mindful an individual is during an

exercise deliberately used to evoke a state of mindfulness. In the Brown and Ryan (2003) study however, the authors use state mindfulness to refer to how mindful an individual is at certain points throughout their everyday life. Perhaps it is unsurprising therefore that these two 'versions' of state mindfulness relate differently to trait mindfulness.

The TMS-State assesses mindfulness at a single time point, and is unlikely therefore to be consistent over time. Situational factors such as how fatigued or stressed an individual is when completing this measure will influence results. This limits its use as an outcome measure and the psychometric properties reflect this. Only one of the subscales of the TMS-State was predictive of better clinical outcome after a mindfulness intervention in terms of stress and psychopathology (Lau et al., 2006).

So, whether or not an individual's ability to achieve a state of mindfulness relates to their level of everyday mindfulness is yet to be established. This is clearly an important issue and further research would perhaps help to shed light on whether this distinction is a useful one. For instance, although state mindfulness may influence an individual's willingness to practice mindfulness exercises, or indeed opt in to and complete a mindfulness intervention, what is arguably more useful for clinicians is the extent to which a mindfulness intervention creates lasting change in a person's everyday level of mindfulness.

#### **2.4.2. Implications for research and clinical practice**

This review has a number of implications for further research and clinical practice. With regard to existing measures of mindfulness, research should focus on validating measures for wider populations. The measures rely heavily on data from student samples and whilst this may provide some adequate normative data, there is a significant lack of mindfulness measures which have been validated for clinical groups. In particular, the current research for clinical groups is often based on small sample sizes and so future research addressing this issue should ensure adequate power is obtained. Despite the lack of research in clinical groups, these questionnaires are frequently applied to measure outcomes with a wide range of

populations and this may be somewhat premature. Certainly, it would be very helpful if this data were published, perhaps with the original authors collating data from multiple centres.

Future studies investigating measures of mindfulness should focus on establishing test-retest reliability, particularly as most current measures of mindfulness claim to measure mindfulness as a stable trait. This would also serve to further our understanding about mindfulness as a psychological construct. Establishing adequate criterion validity should also be a priority. A significant weakness of many of the reviewed studies here is that criterion validity was supposedly established by comparing the measure with an existing measure of mindfulness which was based on a different theoretical perspective of mindfulness. Given that no "gold standard" measure of mindfulness exists, it may be useful for research to focus on investigating relationships between self-report measures and neuropsychological, neuroimaging or behavioural markers of mindfulness, in addition to integrating findings from qualitative research.

Measures of mindfulness could be improved by removing negatively worded items, and ensuring the clarity of items for non-meditators. Finally, although further research of measures is important, a greater priority is to establish an agreed operational definition of mindfulness which is based on a theoretical model. This is by no means straightforward. Mindfulness has its roots in Buddhism, not science, and only recently have scientific methods been employed to investigate it as an intervention for psychological problems. According to Hayes and Shenk (2004, p.253), "Mindfulness is a pre-scientific concept, and it is unlikely that any one definition will allow it to enter into scientific discourse unambiguously".

However, whilst the debate about definitions of mindfulness continues, so will incongruent questionnaires attempting to measure it struggle to do so. Perhaps this is not as problematic as it seems because as with any complex concept researchers, clinicians, and teachers, as individuals, will especially emphasise some aspects and deemphasise others. To some extent the problem here may be that their work is not

accurately evaluated if they use specific mindfulness measures that do not reflect their particular view and practice of mindfulness.

#### **2.4.3. Strengths and limitations of the review**

A strength of this review was that attempts were made to limit publication bias. First authors of all included measures were contacted to identify unpublished measures of mindfulness. Similarly, a great deal of effort was made to ensure literature searches were thorough and systematic (see section 2.2.3). However, there are also a number of limitations. There is the potential for subjective bias in the selection and rating of individual measures, which could have been addressed by involving a second rater.

The exclusion of studies investigating measures published in languages other than English is also a potential weakness. One measure in particular, the FMI, was originally published in German and therefore much of the psychometric data relating to it has not been reviewed here. The reasonably strict inclusion criteria pertaining to the definition of mindfulness resulted in the exclusion of several measures of related constructs. In order to gain a more thorough overview of measures of mindfulness, future reviews could have wider criteria and investigate similar constructs in more detail.

#### **2.5. CONCLUSION**

This systematic review aimed to provide an overview of the psychometric properties of self-report measures of mindfulness. The findings highlight that these are in the early stages of development and based on somewhat different conceptualisations of mindfulness as a construct. Future research in developing these measures should focus particularly on validating these for a wider range of populations, measuring test-retest reliability and improving criterion validity. However, perhaps more of a priority is to establish an agreed definition, one that has a sound theoretical basis which clarifies for example the role or distinction between acceptance, self-compassion and mindfulness. The issue of whether or not mindfulness is psychometrically a multifaceted construct continues to cause debate in the area and this is perhaps inevitable whilst myriad definitions are proposed.

Ultimately self-report measures are an indirect and inaccurate way to measure internal experiences and how we relate to them. On the one hand there is arguably a need for greater agreement and clarity on exactly what we mean by the term "mindfulness", but on the other there is a cogent argument that we are in danger of being reductionist. Certainly for the purpose of clinical practice, what is possibly more important is to be able to identify and measure the psychological processes of mindfulness which mediate positive outcomes. This would allow us to tailor and improve interventions with the hope of increasing their efficacy.

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### 3. META-ANALYSIS: ABSTRACT

**Objective:** The aim of this review was to investigate the efficacy of Mindfulness-Based Stress Reduction (MBSR) for improving quality of life in individuals with chronic health conditions.

**Methods:** A literature search was conducted in February 2011 using the following databases: AMED; PsycINFO; MEDLINE; and EMBASE. Effect sizes were calculated using Hedges'  $g$  and a random effects meta-analysis was conducted.

**Results:** The search identified 19 controlled and observational studies which met the inclusion criteria. These included a total of 1424 participants with a range of chronic conditions. MBSR led to greater improvements in quality of life compared to a control condition and the effect size was small to moderate (Hedges  $g=0.26$ ). Within group effect sizes were also calculated indicating that MBSR had a moderate effect pre to post treatment (Hedges  $g=0.45$ ) and at follow-up (Hedges  $g=0.63$ ).

**Conclusion:** These findings suggest that MBSR does have a positive and significant effect on quality of life for individuals with chronic health problems. These effects appear to be maintained at follow-up. However, publication bias and ceiling effects may have been present and there was some evidence to suggest that MBSR was not more effective than active treatment controls.

*Keywords:* Mindfulness, quality of life, chronic condition, physical health, meta-analysis, mindfulness-based stress reduction, MBSR

#### 4. INTRODUCTION

Chronic physical health conditions, such as cardiovascular disease, cancer and diabetes, are the leading cause of death and disability worldwide (WHO, 2008). In 2001, it was estimated that there were up to 17.5 million adults living with a chronic disease in the UK alone (Department of Health, 2001). Chronic conditions, by their very nature, are enduring, have a pattern of recurrence or deterioration, and have a poor prognosis. Increasingly, it is recognised that health care provision for these individuals is as much about improving quality of life as it is about continuous and complex management of symptoms.

The World Health Organisation (WHO) defines health as “a state of complete physical, mental and social well-being and not merely the absence of disease and infirmity” (WHO, 1947). Studies indicate that chronic health problems impact significantly on all of these aspects of well-being. Regarding the physical impact, a recent review of the literature concluded that physical function was “severely and negatively affected” by chronic disease (Hopman *et al.*, 2009, p. 108). In their study of 1574 patients Bayliss *et al.* (2004) found that congestive heart failure, diabetes and chronic respiratory disease predicted a clinically significant decline in physical functioning, as did the presence of multiple co-morbid chronic conditions.

Perhaps less obvious are the effects of chronic conditions on social well-being and mental health. Although mental health seems to be the domain least likely to be affected by chronic illness (Hopman *et al.*, 2009; Stewart *et al.*, 1989; van't Spijker *et al.*, 1997) it is now well established through research that people suffering from chronic health problems may be more at risk of psychological distress. The probability of having depression, for example, is significantly higher with the presence of a chronic health condition (Egede, 2007; Moussavi *et al.*, 2007). NICE (2009) report that around 15 to 25 per cent of people with conditions such as coronary heart disease, diabetes, cancer and stroke met the diagnostic criteria for depression and that depression was about two to three times more common in these patients than in people without physical health problems. Mental health has also been

found to impact on adherence to treatment and health behaviour recommended for the physical condition (Prince *et al.*, 2007; Sabaté, 2003).

Chronic illness can also affect social functioning by impacting on aspects such as marital relationships, parental responsibilities and social activities (Hahn *et al.*, 2010; Verhaak *et al.*, 2005). Michael *et al.* (2000) compared the functional health status of over 700 women diagnosed with breast carcinoma to healthy controls and found that a decline in both physical and social function was present up to four years post-diagnosis. In a large scale study, Stewart *et al.* (1989) found that chronic conditions had a marked negative impact on physical, role and social functioning, with myocardial infarction and congestive heart failure having the greatest impact on social functioning.

These physical, psychological and social factors are by no means independent. Increasingly, a biopsychosocial approach to understanding chronic disease is being adopted and evidence from studies investigating the association between psychological and biological factors has strengthened this view. For example, a comprehensive review by Gallo & Matthews (2003) highlighted that negative affect can predict physical health outcomes. In particular, they point to convincing research in the field of cardiovascular morbidity and mortality, both of which appear to be significantly affected by depression, hopelessness and hostility. Penninx *et al.* (2001) found that in a sample of over 400 patients with cardiac disease, depression was associated with an increased risk of cardiac mortality by about 1.6 times for older adults with minor depression and three times in those with major depression. However, as noted by Goodwin *et al.* (2009), methodological weaknesses, a lack of longitudinal studies and poor understanding of mechanisms make the research linking physical and mental health difficult to interpret.

The fact that chronic illness impacts not just on a person's physical health but also on other aspects of their functioning and well-being, has had implications for outcome research in this area. Outcome measures such as quality of life have commonly been used as markers for change following medical and psychological interventions in this

population. Although a widely used term, 'quality of life' remains an ambiguous concept. Definitions include:

- “the subjective sum of multiple physical, emotional, social and objective dimensions of one’s life” Bowling (2005, p.125)
- “an individual’s perception of their position in life in the context of the culture and value systems in which they live and in relation to their goals, expectations, standards and concerns” (WHOQOL, 1997, p.1)
- “patients’ appraisal of and satisfaction with their current level of functioning as compared to what they perceive to be ideal’ (Cella & Tulsky, 1990, p.29)

In common with most definitions is the fact quality of life is multidimensional, subjective, and dynamic (i.e. changing over time) including aspects of an individual's physical, functional, emotional and social well-being (Cella, 1994; Speight *et al.*, 2009). Perhaps because it is multidimensional, it has been widely used as an outcome measure to evaluate interventions for people with chronic health problems.

A distinction has been made between quality of life and health-related quality of life; the latter encompassing an individual’s perception of the impact of their illness and its treatment(s) (Speight *et al.*, 2009). Consequently, health-related quality of life has been developed to describe aspects of an individual’s subjective experience that relate both directly and indirectly to health, disease, disability, impairment, and to the effectiveness of treatment (Carr *et al.*, 2001). In practice, these terms are used interchangeably and particularly in chronic health populations, Moons *et al.* (2006) argue that a distinction is irrelevant as it would be difficult, if not impossible, for a person to distinguish between parts of their life influenced by their health or not.

It is recognised that chronic physical health conditions impact significantly on an individual's quality of life and increasingly psychological interventions are being considered to address this. Several clinical guidelines currently recommend psychosocial interventions such as individual and group based cognitive behavioural therapy, relaxation training and psychoeducation (SIGN, 2002, 2004, 2008; NICE,

2009). One type of intervention which is increasingly being applied in chronic health settings to improve quality of life is mindfulness-based therapy.

Mindfulness-Based Stress Reduction (MBSR) is a therapeutic intervention originally developed for patients with chronic pain and stress-related disorders (Kabat-Zinn, 1990). Based on the principles of mindfulness meditation, it encourages participants to focus their attention on the present moment and adopt an attitude of acceptance and openness to that experience, be it positive or negative. The intervention typically consists of eight weekly group sessions and mindfulness meditation is taught using exercises such as breath awareness, body scan and gentle stretching. Participants are provided with meditation exercises on CD to practice at home and specific homework tasks are set each week.

One of the most distinguishing features of mindfulness as a psychological intervention is the emphasis on acceptance of negative cognitive or physical experiences, rather than seeking to change or distract from them. This attitude of acceptance may be particularly important for individuals whose chronic health problems cause a host of negative psychological and physical symptoms (Reibel *et al.*, 2001).

Empirical research of mindfulness-based therapies is growing and several meta-analyses published in the last five years show promising results. Most recently, Hofmann *et al.* (2010) investigated the effect of mindfulness-based therapies (i.e. not exclusively MBSR) in a heterogeneous sample of people suffering from clinical levels of anxiety or depression. A moderate effect sizes from 39 studies was found for symptoms of anxiety (Hedges'  $g=0.63$ ) and depression (Hedges'  $g=0.59$ ). Bohlmeijer *et al.* (2010) found small to moderate effect sizes for anxiety, depression and psychological distress in adults with chronic medical diseases across eight controlled studies. Similar findings are reported for the effect of MBSR reducing anxiety and depression in cancer patients (Ledesma & Kumano, 2009) and reducing stress in a healthy population (Chiesa & Serretti, 2009).

## **Rationale for meta-analysis**

It seems clear from the evidence that mindfulness has small to moderate effects on reducing psychological distress in a wide range of populations. However, no prior reviews have investigated the effect of MBSR on quality of life. Consequently, the purpose of the current meta-analysis was to determine the efficacy of MBSR for improving quality of life in people with chronic health problems. This study will therefore focus on an aspect of the mindfulness literature which has not been investigated by previous reviews.

Previous reviews of MBSR have focussed on the extent to which symptoms such as anxiety and depression are improved. Mental health has long been defined as the absence of psychopathologies such as anxiety and depression (Westerhof & Keyes, 2010). However, the dual-continua model of mental health proposes that mental health and mental illness represent distinct, but obviously related, dimensions: that is, the absence of mental illness does not necessarily imply mental health and vice versa (Keyes, 2002). Confirmatory factor analysis in a large scale study supports this theory (Keyes, 2005). Mental health therefore, is best viewed as a complete state including the absence of mental illness and the presence of mental health (Friedli, 2009). Following on from this, it is increasingly recognised that reducing psychological symptoms is a minimal outcome for psychological therapies. This may be particularly true of chronic illness, which we know impacts on many aspects of functioning and well-being and not just on mental health.

Quality of life was chosen as an outcome measure as it was considered to better reflect the impact of chronic illness, and therefore the extent to which this population may benefit from MBSR, than simply the presence of psychological distress. As highlighted, chronic illness appears to be associated with a reduction in physical, social and mental well-being and functioning. The fact quality of life is a multi-dimensional construct is perhaps why it has become such an important, and clinically relevant, indicator of health over the last few decades (Arnold *et al.*, 2004).

Mindfulness interventions may also be more accurately evaluated by the use of multi-dimensional constructs as these interventions emphasise broader outcomes than symptom reduction (Vollestad *et al.*, 2011). MBSR was developed as a "vehicle through which people could assume a degree of responsibility for their own well-being and participate more fully in their own unique movement towards greater levels of health" (Kabat-Zinn, 2003, p.149). This emphasis on improving well-being, and not solely reducing stress, is reflected in the outcome research of mindfulness based interventions, where MBSR has been associated with improvements in well-being (Brown *et al.*, 2003), quality of life (Carlson *et al.*, 2003), fatigue (Grossman *et al.*, 2010) and pain (Rosensweig *et al.* 2010). Given the evidence which suggests mindfulness interventions may have a role to play in a wide variety of positive outcomes, and the fact these interventions emphasis broader outcomes than symptom reduction, quality of life was chosen as suitable outcome measure to assess the efficacy of MBSR.

## 5. METHODS

### 5.1. Inclusion and exclusion criteria

#### 5.1.1. Participants

Studies were included if participants were adults over the age of 18 with a chronic health condition. The term 'chronic condition' is a term often used ambiguously in the literature and it was therefore necessary to decide on a working definition for the purposes of this research. Frequently, chronic conditions are considered 'chronic' depending entirely on the duration of the illness. However, as identified in a review of the chronic illness literature by O'Halloran *et al.* (2004), several other factors may be important indicators. Examples of these are: the extent to which symptoms recur or worsen over time; the emphasis of treatment being on managing, rather than curing, the condition; and whether or not the condition causes sequelae which impact on a person's quality of life. Given the aim of this review was to determine the efficacy of MBSR for improving quality of life, it was important to consider conditions which were not only long-lasting but also those which have a detrimental impact on quality of life in the first place.

Therefore a chronic condition was defined as a condition which has:

- a) A duration that has lasted, or is expected to last, at least 6 months
  - b) A pattern of recurrence or deterioration
  - c) A poor prognosis
  - d) Consequences or sequelae that impact on the individual's quality of life
- (O'Halloran *et al.*, 2004, p.384).

This study focussed only on those conditions which met all of these criteria and were considered to be physical health conditions. Bohlmeijer *et al.* (2010, p.540) define physical health conditions as “*any conditions which involve some disability, caused by irreversible pathological change... [including]... illnesses that need not be irreversible but cause enduring disability (e.g., cancer).*” Clearly, a significant degree of overlap exists between whether a condition is physical or psychological in nature, the obvious example being that of psychosomatic conditions. However, studies which included participants with a predominant diagnosis of anxiety, stress, depression or psychological distress, without mention or emphasis of the physical



sequelae of these conditions, were excluded from this review. Studies were therefore included if participants had a diagnosable physical health condition (as described above and by Bohlmeijer *et al.*, 2010) which was considered to be chronic in nature, based on O'Halloran's four criteria.

#### *5.1.2. Intervention*

Studies were included if they investigated Mindfulness-Based Stress Reduction (MBSR), the programme developed by Kabat-Zinn and colleagues (Kabat-Zinn, 1990). There are a number of different mindfulness based interventions described in the literature, but in order to increase the generalisability of the review MBSR studies only were included. The MBSR intervention had to be delivered as weekly group therapy of at least six weeks duration. Studies where the intervention was 'based on' MBSR were also included so long as the previous two conditions were met (i.e. group therapy and at least six weeks duration) and where the predominant therapeutic approach was MBSR. Studies were excluded if the intervention was delivered remotely, for example via the internet as an important component of MBSR as it is described in the manual is the fact it is a group based intervention.

#### *5.1.3. Outcomes*

This meta-analysis examined studies where the outcome measure was quality of life. There is no agreed definition of quality of life but there is broad agreement in the literature that it is a multidimensional construct encompassing the subjective evaluation of the physical, psychological and social aspects of one's life (Bowling, 2005; Speight *et al.*, 2009). Therefore, studies were included if they utilised a self-report measure, pre and post intervention, which assessed quality of life in terms of all three of these domains. Single item measures, such as visual analogue scales, were excluded as they do not sufficiently assess the multidimensional nature of quality of life (Cella, 1994).

#### *5.1.4. Study types*

Only quantitative studies published in English were considered for this review. Although both controlled and uncontrolled trials were included (in order to identify

as many studies as possible in an area where research is still relatively limited) uncontrolled studies were analysed separately to account for the variability of methodological quality in these two types of design. Studies were excluded if they did not publish sufficient data to allow for the calculation of an effect size (e.g. means, standards deviations or test statistics). Efforts were made to contact authors to request this information.

## **5.2. Search strategy**

The search strategy was designed to identify all studies investigating MBSR as an intervention for people with chronic health conditions. The search was conducted in February 2011 by the first author, after consultation with a librarian experienced in database searches. The following databases were searched: AMED (1985-Feb 2011); EMBASE (1947-Feb 2011); MEDLINE (1950-Feb 2011); and PsycINFO (1806-Feb 2011) using OVIDSP. The search terms used were: ('mindful\*' OR 'MBSR' OR 'meditation' OR 'stress reduction') AND ('chronic' OR 'physical' OR 'medical' OR 'health' OR 'disease' OR 'condition') AND ('quality of life' OR 'well-being' OR 'life satisfaction'). These terms were searched within the domains of title, abstract and key words.

The contents pages of the following journals were searched for relevant articles by using a Boolean search with the term 'mindful\*' anywhere in the article titles: Journal of Alternative and Complimentary Medicine (1998 to 2011); Journal of Psychosomatic Research (1960 to 2011); British Journal of Health Psychology 1998-2006; Psychosomatic Medicine (1939 to 2011) and relevant articles were screened based on the title and abstract. These journals were selected after scanning the articles identified from the database search and searching the most prevalent publications.

The reference lists of included studies were also screened for relevant articles and the 18 different first authors contacted in order to identify any unpublished research. A response was received by six and six studies were suggested by these authors, two of which did not meet the inclusion criteria and four of which had been submitted for

publication and could therefore not be released for inclusion in this review at the authors' insistence.

### **5.3. Assessment of study quality**

Each study was coded for methodological quality using a set of criteria which were developed specifically for this review. These were used to identify weaknesses in the research that might bias the results and to identify ways in which studies may mask or exaggerate the effectiveness of an intervention when it is not conducted rigorously (CRD, 2009). Guidance given by SIGN (2008) and scales used by other authors for similar reviews were considered in the development of these criteria. Given that no single tool exists to rate the quality of both controlled and uncontrolled studies, it was necessary to develop criteria specifically for this review as recommended by the Centre for Reviews and Dissemination (CRD, 2009).

The aim was to systematically assess the internal and external validity, including an assessment of outcome measurement, the quality of the intervention delivered, the allocation of participants to groups and the statistical analysis of the data in primary studies. Whilst this is by no means an exhaustive list, the quality criteria were selected to reflect key methodological issues pertinent to the research in the field of mindfulness. The Cochrane criteria for the assessment of risk of bias in randomised control trials initially served as the basis for the criteria selected (CRD, 2009, p. 37). All of their recommended criteria were included apart from two relating to a) the blinding of care providers, participants and outcome assessors, and b) selective bias in outcome reporting. Blinding was excluded from this review due to the recognised difficulties in blinding participants in psychological interventions (Roberts *et al.*, 2009) and the fact outcome measurement in primary studies was self-reported as opposed to clinician rated. The second criteria was not considered relevant as inclusion criteria had already selected studies which included quality of life as an outcome measure. Whilst it is acknowledged that bias in reporting may still be an issue in the primary studies, this was not considered to be a key issue which would influence effect sizes in a meta-analysis.

An additional criterion was included which was the presence (and quality) of the control group. This was included because both controlled and uncontrolled studies were included in this review and was considered to be important in evaluating the quality of evidence in primary studies. The Cochrane Collaboration also highlight the importance of the choice of outcome measure, quality of the intervention, statistical issues, quality of reporting and the generalisability of the research, when assessing study quality. As such, an additional six criteria, drawn from similar reviews were selected to address these factors (Cartwright-Hatton *et al.*, 2004; Hofmann *et al.*, 2010; and Ost, 2008). These were: calculation of power, experience of therapist, description of intervention, therapist effects, reliability of outcome measure, and presence of follow-up. Although quality of reporting was not assessed as a separate criterion, when a study did not report enough information to decide whether or not a criterion was met this was recorded as 'not reported'. Generalisability of the primary studies, although not coded for specifically, was considered more generally in the discussion.

The final quality criteria were therefore as follows:-

- a) Control: Treatment condition was compared to that of a suitable control (e.g. treatment as usual (TAU), wait-list control (WLC) or another empirically documented treatment).
- b) Group similarities: Patient characteristics (age, sex, disease status, quality of life pre-intervention scores) were similar in the treatment and control condition prior to the intervention and if not this difference was controlled for statistically.
- c) Allocation: Patients were allocated randomly to groups.
- d) Diagnosis was confirmed by a trained clinician prior to enrolment in the study.
- e) Attrition rate was 10% or less in each group(s).
- f) Appropriate intention to treat analysis was used.
- g) A power calculation was reported and sample size was decided and obtained accordingly.
- h) Treatment was delivered by suitably trained, experienced therapists.
- i) Treatment was manualised, replicable and specific.

- j) Efforts were undertaken to minimise individual therapist effects.
- k) Quality of life outcome measure was deemed to be valid and reliable, and Cronbach's  $\alpha \geq 0.7$ .
- l) Outcome measurement took place pre and post intervention, and at least 3 months after the intervention was complete.

Each study was coded on each criterion using a scale of two points for 'well addressed'; one point for 'adequately addressed'; and zero points for 'poorly addressed', 'not applicable', or 'not reported'. All studies were coded by the first author and a random sample of nine studies was coded by the second author. Ratings were compared for consistency between authors and amendments made where appropriate.

#### **5.4. Statistical analysis**

The standardised mean difference was calculated for each study using Hedge's  $g$ , a version of Cohen's  $d$  adjusted to minimise the effect of bias from small sample size. Both within group differences and between group differences were calculated for continuous measures of quality of life. These were calculated in Microsoft Excel using the formulas described in Appendix 6. The magnitude of Hedges'  $g$  was interpreted using the convention: small (0.2), medium (0.5), and large (0.8) (Cohen, 1988).

The correlation between pre and post values for within group differences was required in order to calculate effect sizes. As this information was not reported in studies, a conservative estimate of  $r=0.7$  was used for all calculations (Rosenthal, 1993 as cited in Hofmann *et al.*, 2010; also see Grossman *et al.*, 2004).

A random effects meta-analysis was then carried out using the Cochrane Collaboration Review Manager Software (RevMan version 5.0). This involves calculating a weighted mean of effect sizes where the effect size in each study is weighted by the inverse of its variance. A test of the null hypothesis that the mean effect is zero was conducted and the data tested for heterogeneity. When evidence of

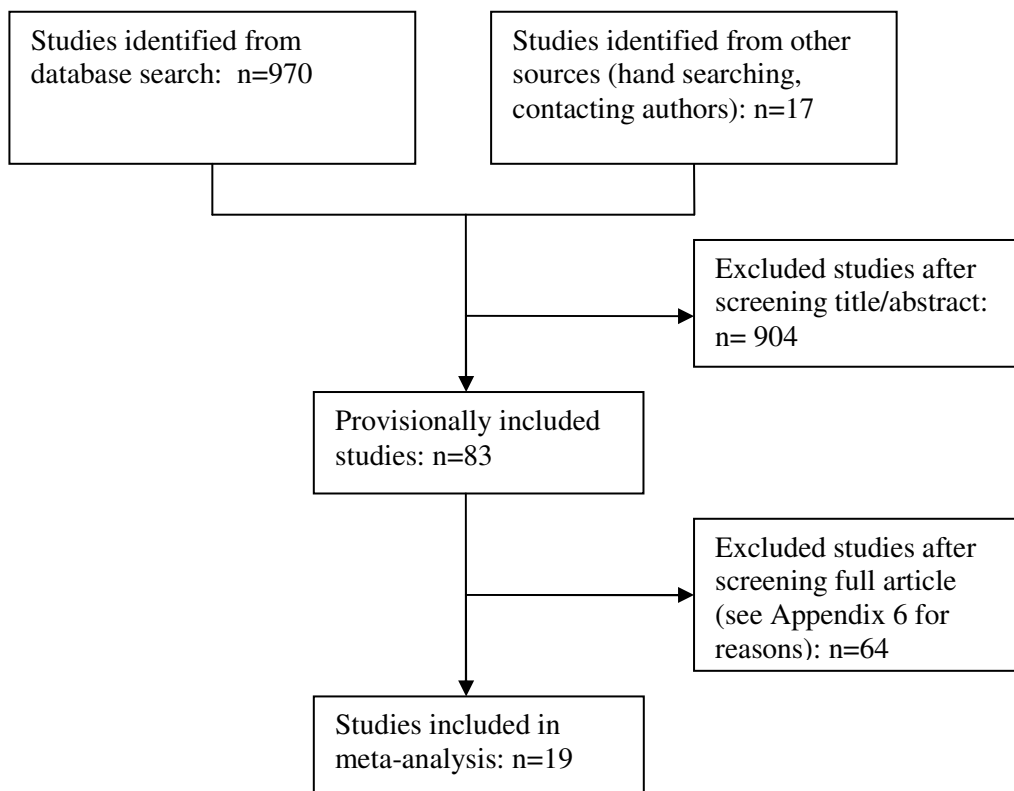
heterogeneity was present amongst effect sizes, these were explored using subgroup analysis.

Meta-analyses may be at risk of overestimating overall effect sizes because studies with insignificant results are less likely to be published. This publication bias has been termed the 'file drawer problem' (Rosenthal, 1979). In this review, publication bias was assessed using a funnel plot and by calculating the *fail safe N* (see Appendix 6 for details).

## 6. RESULTS

### 6.1. Overview of included studies

A total of 987 articles were identified from the search process described. After screening the abstract and title of all studies identified, 904 studies were excluded and the full articles of the remaining 83 studies were retrieved. Of these, 19 studies met the inclusion criteria and were included in the meta-analysis. The flow chart in Figure 6.1. describes the process of identifying studies. The reasons for studies being excluded from this review are detailed in Appendix 7 and summarised in Table 6.1.



**Figure 6.1.** Flowchart illustrating literature search process

The 19 studies included in the review are summarised in Table 6.2. Collectively, these studies investigated mindfulness with 1424 participants (72% female), with a mean age of 51.8 (SD=9.38). Six studies involved an entirely female sample (Grossman *et al.*, 2007; Lengacher *et al.*, 2009; Ljótsson *et al.*, 2010; Monti *et al.*, 2006; Schmidt *et al.*, 2011; Witek-Janusek *et al.*, 2008).

**Table 6.1.** Reasons for studies being excluded from review

Reason	N*
No QoL measure used	26
Not a chronic health population	11
Intervention was not MBSR	10
Not a quantitative study	8
Not enough data reported	7
Unable to access full text	3
Study not in English	2
Same data set as another included study	1
Subjects under age of 18	1

\*Some studies were excluded for more than one reason

Studies were primarily controlled trials (74%), and used a wait-list control (WLC), treatment as usual (TAU), or an active control (AC). One study used the participants who dropped out of the mindfulness intervention as a control group (Bédard *et al.*, 2003). The most common disorder studied was chronic pain or fibromyalgia, followed by cancer. Ten studies measured quality of life after a follow-up period, which ranged from 1 month to 3 years post intervention (M=39.9 weeks, SD=44.9).

Fourteen studies (74%) investigated MBSR whereas others investigated an intervention which was ‘based on’ MBSR, but included some additional element. For example Monti *et al.* (2006) investigated Mindfulness-Based Art Therapy (MBAT) and Gardner-Nix *et al.* (2008) investigated Mindfulness-Based Chronic Pain Management (MBCPM).

Twelve different self-report outcome measures were used to measure quality of life and the domains covered by these, used to determine eligibility for inclusion, are summarised in Appendix 5. The most frequently used measure was the Medical Outcomes Survey–Short Form 36 (SF-36) and 7 (37%) studies used this measure. Outcome measures were either generic (50%) or disease specific (50%) with all but three having adequate internal consistency.



**Table 6.2.** Overview of included studies

Study	Medical Condition	Groups	N <sup>a</sup>	Mean age in years (sd)	QoL measure	Follow-up period	Within group, pre-post effect size <sup>b</sup>	Within group, pre-follow up effect size <sup>b</sup>	Between group effect size <sup>b</sup>
Astin <i>et al.</i> (2003)	Fibromyalgia	MBSR Education group	32 33	47.7 (10.6)	FIQ	24 wks	0.63	0.61	0.19
Bedard <i>et al.</i> (2003, 2005)	Brain injury	MBSR Drop-outs	10 3	43 39	SF-36	1 yr	1.06	0.86	1.06
Carlson <i>et al.</i> (2003, 2007)	Cancer	MBSR	59	54.5 (10.9)	EORTC QLQ-C30	1 yr	0.26	0.29	no control group
Cusens <i>et al.</i> (2010)	Chronic pain	MBPM TAU	32 20	46.7 (11.5) 48.4 (12.3)	SF-36	none	0.30	n/a	-0.25
Gardner-Nix <i>et al.</i> (2008)	Chronic pain	MBPM WLC	99 57	51 52	SF-36	none	0.24	n/a	0.26
Gross <i>et al.</i> (2010)	Solid organ transplant	MBSR Education group	63 59	55 (11.3) 52 (10.4)	SF-12	1 yr	0.74	0.84	0.07
Grossman <i>et al.</i> (2007)	Fibromyalgia	MBSR Support group	39 13	54.4 (8.3) 48.8 (9.1)	QOL	3 yrs	0.92	0.61	0.83
Grossman <i>et al.</i> (2010)	Multiple Sclerosis	MBI TAU	76 74	45.9 (10.0) 48.7 (10.6)	QOL HAQUAMS	none	0.37	n/a	0.63
Knauss (2007)	Cancer	MBSR	20	Range 31-70	FACT-G	none	-0.05	n/a	no control group
Lengacher <i>et al.</i> (2009)	Cancer	MBSR TAU	40 42	57.5 (9.4)	SF-36	none	0.38	n/a	0.22
Ljotsson <i>et al.</i> (2010)	IBS	MBSR	34	34.6 (11.0)	IBS-QOL	6 mnths	1.37	1.3	no control group
Monti <i>et al.</i> (2006)	Cancer	MBAT WLC	56 55	53.1 (12.4) 54.1 (10.7)	SF-36	none	0.31	n/a	0.33
Morone <i>et al.</i> (2008)	Chronic pain	MBSR WLC	19 18	74.1 (6.1) 75.6 (5.0)	SF-36	3 mnths	0.33	0.31	0.12

Study	Medical Condition	Groups	N <sup>a</sup>	Mean age in years (sd)	QoL measure	Follow-up period	Within group, pre-post effect size <sup>b</sup>	Within group, pre-follow up effect size <sup>b</sup>	Between group effect size <sup>b</sup>
Mularski <i>et al.</i> (2009)	Chronic obstructive lung disease	MBBT Support group	44 42	70.6 (10.6) 64.0 (9.1)	Veterans SF-36	none	-0.06	n/a	-0.15
Robinson <i>et al.</i> (2003)	HIV	MBSR TAU	23 10	43.1 (6.1) 36.1 (8.0)	FAHI	none	0.32	n/a	0.18
Rosenzweig <i>et al.</i> (2010)	Chronic pain	MBSR	99	49.8	SF-36	none	0.36	n/a	no control group
Schmidt <i>et al.</i> (2011)	Fibromyalgia	MBSR Relaxation (AC) WLC	53 56 59	53.4 (8.7) 51.9 (9.2) 52.3 (10.9)	QOL FIQ	8 wks	0.45	0.36	0.2(AC) 0.4(WLC)
Surawy <i>et al.</i> (2005)	Chronic fatigue syndrome	MBSR (1) MBSR (2)	10 9	18-65	FIS	3 mnths	0.73	0.67	no control group
Witek-Janusek <i>et al.</i> (2008)	Breast cancer	MBSR WLC	38 28	55 (10) 54 (8)	QLI	1 mnth	-	n/a	0.58

AC: Active control; MBSR: Mindfulness-based Stress Reduction; MBPM: Mindfulness-based Pain Management; MBI: Mindfulness-based Intervention; MBAT: Mindfulness-based Art Therapy; MBBT: Mindfulness-based Breathing Therapy; TAU: Treatment as usual; WLC: waitlist control

EORTC QLQ-C30: European Organisation for Research and Treatment of Cancer Quality of Life Questionnaire; FACT-G: Functional Assessment of Chronic Illness Therapy-General; FAHI: Functional Assessment of HIV Infection; FIQ: Fibromyalgia Impact Questionnaire; FIS: Fatigue Impact Scale; HAQUAMS: Hamburg Quality of Life Questionnaire in Multiple Sclerosis; IBS-QOL: Irritable Bowel Syndrome-Quality of Life Measure; QLI: Quality of Life Index; QOL: Profile of Health-Related Quality of Life in Chronic Disorders; SF-12: Short Form-12 Health Survey; SF-36: Short Form-36 Health Survey; Veterans SF-36: Veterans Short Form-36

<sup>a</sup> N is the number of people for whom data were reported in the study, and therefore who were entered into the final analysis. This is not the number of people recruited for the study and therefore does not include those who dropped out.

<sup>b</sup> Effect size is Hedge's *g* (see Section 5.4. for calculations used)

## 6.2. Study quality

Study quality was assessed using a set of criteria developed specifically for this review (see Section 5.3). The detailed results for study quality ratings are shown in Table 6.3. A random selection of nine studies was reviewed by a second rater. The two raters independently assigned the same methodological quality score for 84/101 (83%) of ratings, and differed by one point on 15/101 (15%) and by two points on 2/101 (2%) of ratings. Where differences between ratings of the first and second author occurred, these were discussed and changes made where it was deemed appropriate to do so.

Overall, the average quality score was 13.3 (SD 5.6), out of a possible 24 points. Studies rated with the highest quality score were Grossman *et al.* (2010) with a score of 23, Schmidt *et al.* (2011) with a score of 20 and Monti *et al.* (2006) with a score of 20. Most studies used a manualised, replicable and specific intervention, as most used MBSR as based on the manual by Kabat-Zinn (1990). Where studies included additional elements, the intervention was described in sufficient detail.

Likewise, all studies scored either well or adequately addressed the criterion relating to the use of a valid and reliable quality of life outcome measure. This was commonly used as a secondary outcome measures and therefore few studies reported or commented on the reliability and validity of these measures. Therefore, the psychometric properties had to be obtained independently.

Other criteria which were satisfactorily addressed by 68% of the studies were the use of an adequate control group; the groups being similar prior to the intervention (68%); and the therapists having suitable training and experience to deliver the intervention (79%). However, there was a significant amount of variability in the quality of control conditions. One study used dropouts from the MBSR as the control group and had only three participants in this group (Bédard *et al.*, 2003), and others used an active control which was not an empirically validated treatment, such as a support group or health education control condition. Treatment and control groups were largely similar in terms of demographics prior to the intervention, though

several studies did not report on whether quality of life scores were statistically different at baseline.

Criteria which were least likely to be well or adequately addressed were attrition, power and therapist effects. Over half of the studies reported an attrition rate of over 10%. A power calculation was not reported for 58% of studies and efforts to reduce individual therapist effects were not addressed by 58% of studies.

**Table 6.3.** Study quality ratings

Study/Quality criteria	Control	Group similarities	Allocation	Diagnosis	Attrition	Intention-to-treat	Power	Therapist experience	Treatment	Therapist effects	Outcome measure	Follow-up	Quality score	Overall rank
Astin <i>et al.</i> (2003)	2	2	2	2	0	0	2	1	2	0	1	2	16	8
Bedard <i>et al.</i> (2003, 2005)	0	0	0	0	0	0	0	0	2	0	2	2	6	16
Carlson <i>et al.</i> (2003, 2007)	0	0	0	1	0	0	0	0	2	0	2	2	7	15
Cusens <i>et al.</i> (2010)	2	2	0	0	2	2	0	2	2	1	2	0	15	9
Gardner-Nix <i>et al.</i> (2008)	2	1	0	0	0	0	0	0	1	0	2	0	6	16
Gross <i>et al.</i> (2010)	2	2	2	0	2	1	2	1	2	1	1	2	18	4
Grossman <i>et al.</i> (2007)	2	1	1	2	1	0	0	2	2	0	1	2	14	10
Grossman <i>et al.</i> (2010)	2	2	2	2	2	2	2	2	2	1	2	2	23	1
Knauss (2007)	0	0	0	0	0	0	0	2	1	0	2	0	5	19
Lengacher <i>et al.</i> 2009)	2	1	2	0	2	2	2	2	2	0	2	0	17	5
Ljotsson <i>et al.</i> (2010)	0	0	0	2	2	2	0	1	1	1	2	2	13	12
Monti <i>et al.</i> (2006)	2	2	2	2	0	2	2	2	2	0	2	2	20	2
Morone <i>et al.</i> (2008)	2	2	2	0	0	2	0	2	2	1	2	2	17	5
Mularkski <i>et al.</i> (2009)	1	1	2	2	0	2	2	2	1	2	2	0	17	5
Robinson <i>et al.</i> (2003)	2	1	0	0	0	0	2	2	2	0	2	0	11	13
Rosenzweig <i>et al.</i> (2010)	0	0	0	1	0	0	0	2	2	1	2	0	8	14
Schmidt <i>et al.</i> (2011)	2	2	2	2	1	2	2	2	2	1	1	1	20	2
Surawy <i>et al.</i> (2005)	0	0	0	1	0	0	0	0	1	0	2	2	6	16
Witek-Janusek <i>et al.</i> (2008)	2	1	0	2	1	0	0	2	2	0	2	1	13	11
<b>Total graded 2 or 1 (%)</b>	<b>13 (68%)</b>	<b>13 (68%)</b>	<b>9 (47%)</b>	<b>11 (58%)</b>	<b>8 (42%)</b>	<b>9 (47%)</b>	<b>8 (42%)</b>	<b>15 (79%)</b>	<b>19 (100%)</b>	<b>8 (42%)</b>	<b>19 (100%)</b>	<b>12 (63%)</b>	<b>13.3 average</b>	

2=well addressed; 1=adequately addressed; 0=poorly addressed, not applicable, or not reported

### 6.3. Quantitative data synthesis

#### 6.3.1. Between group effect sizes

Fourteen studies compared MBSR to a control condition and these were initially analysed as a whole group. One study compared MBSR to two different control conditions, a waitlist control (WLC) and an active control (AC), and therefore a conservative approach was taken to only include the AC data in this part of the analysis (Schmidt *et al.*, 2011).

Controlled effect sizes were also analysed in two separate groups: those using an active treatment control (N=5) and those using a WLC or TAU (N=10)<sup>1</sup>. The decision to combine WLC with TAU was based on the rationale that people tend to continue receiving other treatment whilst in a waitlist control condition, which would therefore be similar to TAU. All effect sizes refer to the standardised mean difference, calculated using Hedges' *g*, of quality of life scores between the control and treatment group.

Across all controlled studies, MBSR led to greater improvements in quality of life compared to a control condition. A small effect size of  $g=0.26$  (95% CI 0.11, 0.41) was found which was statistically significant ( $Z=3.33$ ,  $p=0.0009$ ). The test of heterogeneity was not significant ( $p=0.12$ ) indicating that the effect sizes were consistent across studies and that further subgroup analysis was not appropriate. These results are displayed in Table 6.4.

Five studies compared MBSR to an active treatment control and although MBSR did lead to greater effects on quality of life than the active control, the effect was small and insignificant. An effect size of 0.16 (95% CI -0.09, 0.41) was found, which was not statistically significant ( $Z=1.28$ ,  $p=0.24$ ). These results are displayed in Table 6.5.

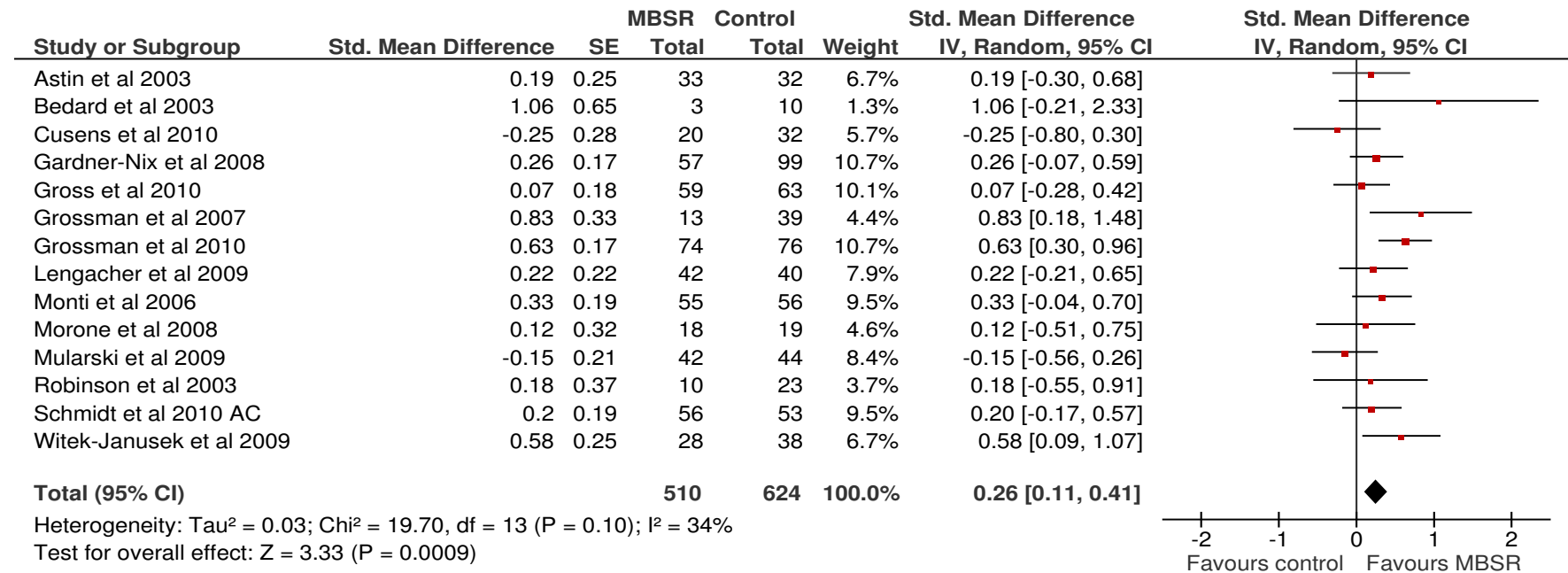
Ten studies compared MBSR with WLC or TAU controls and a small effect size of 0.34 (95% CI 0.18, 0.50) was calculated, which was highly statistically significant

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<sup>1</sup> In this case both sets of data from Schmidt and colleagues was entered, resulting in a total N=15 rather than N=14 for this section.

( $Z=4.18$ ,  $p=0.0001$ ). This indicated that MBSR led to greater improvements in quality of life than either a WLC or TAU condition. The test of heterogeneity was not significant ( $p=0.29$ ) indicating that further subgroup analysis was not appropriate. These results are displayed in Table 6.6.

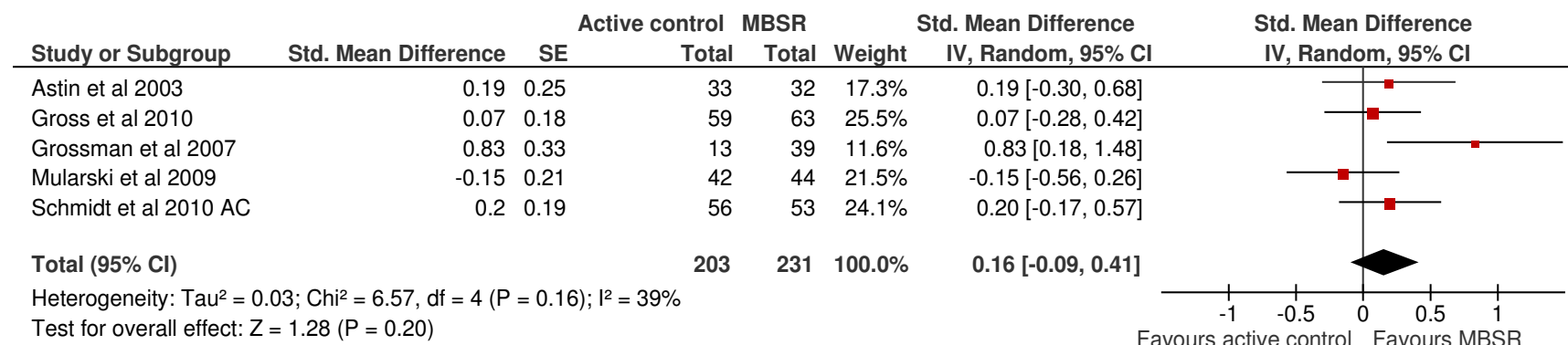
**Table 6.4.** Forest plot showing effects of MBSR on QoL compared to a control condition



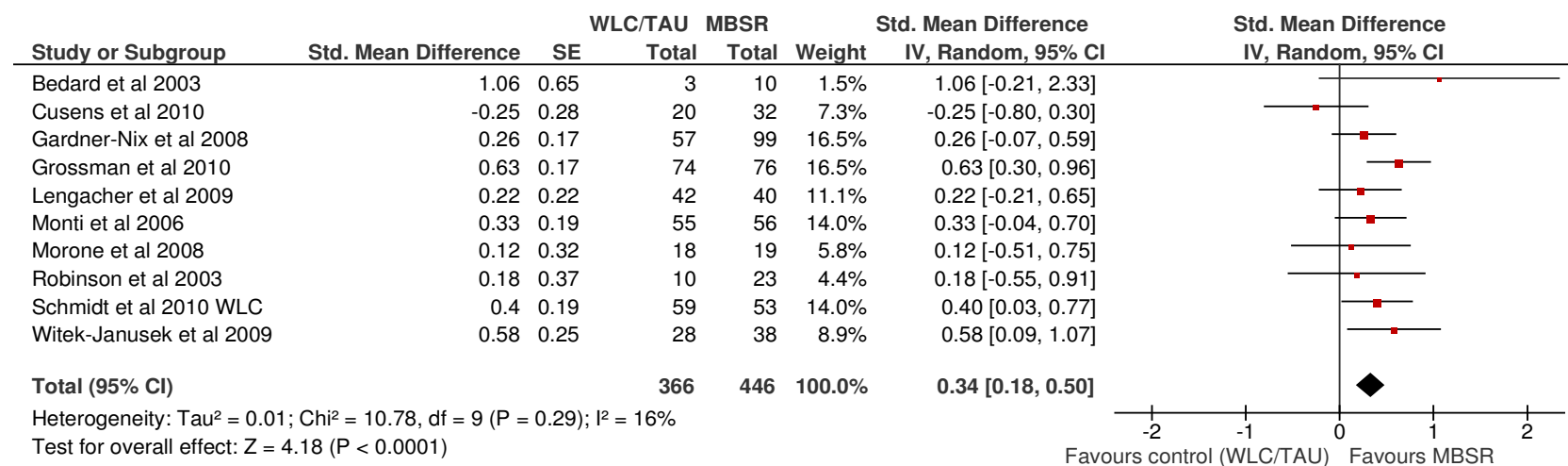
NB. MBSR and Control Total=number of participants in each group; IV: independent variable; random: random effects meta-analysis



**Table 6.5.** Forest plot showing effects of MBSR on QoL compared to active treatment controls



**Figure 6.6.** Forest plot showing effects of MBSR on QoL compared to WLC or TAU controls



### 6.3.2. Within group effect sizes

Data to calculate within group effect sizes was available from 18 out of the 19 studies. One study did not report sufficient pre-post data to calculate an effect size (Witek-Janusek *et al.*, 2008). Effect sizes were calculated for all individuals who received a mindfulness intervention (N=817). This included isolating the mindfulness group data from controlled studies and calculating the pre-post effect sizes for the treatment group only. A medium effect size of 0.45 (95% CI 0.31, 0.59) was calculated, which was highly statistically significant ( $Z=6.24$ ,  $p<0.00001$ ). The test of heterogeneity indicated that there was a significant level of inconsistency across findings ( $p<0.00001$ ) and this suggested that further subgroup analysis was appropriate. Individual effect sizes are displayed in Figure 6.2.

Stem	Leaf
-0.	1,1
0.	2,3,3,3,3,3,4,4,4
0.	5,6,7,7,9
1.	1,4

**Figure 6.2.** Stem and leaf plot of all pre-post within group effect sizes (*g*)

### 6.3.3. Within group effect sizes after follow-up

Ten studies investigated the effect of MBSR after a follow-up period, ranging from one month to three years post intervention. Data for nine studies was used for the analysis of effect sizes as it was not possible to extract sufficient data for one study (Witek-Janusek *et al.*, 2008). A medium effect size of 0.63 (95% CI 0.41, 0.85) was calculated which was highly significant ( $Z=5.62$ ,  $p<0.00001$ ). The test of heterogeneity indicated that there was not a significant level of inconsistency across findings ( $p=0.14$ ).

## 6.4. Subgroup analysis

The test of heterogeneity for within group pre-post effect sizes in the mindfulness groups indicated that there was a significant level of inconsistency in effect sizes across studies (see Section 6.3.2.). In order to investigate possible moderating

variables, correlation and subgroup analysis was used to investigate the relationship between study quality and effect size, and a t-test to explore the differences between types of measure.

#### *6.4.1. Study quality*

A Pearson product-moment correlation was computed to assess the relationship between study quality and pre-post effect size. This was not significant ( $r=-0.02$ ,  $n=18$ ,  $p=0.947$ ).

The impact of study quality on effect size was also investigated by calculating weighted pre-post effect sizes for studies which were considered of higher or lower methodological quality. This involved conducting a sub-group analysis on two data sets: studies where the quality score was above the median and those where the quality score was below the median.

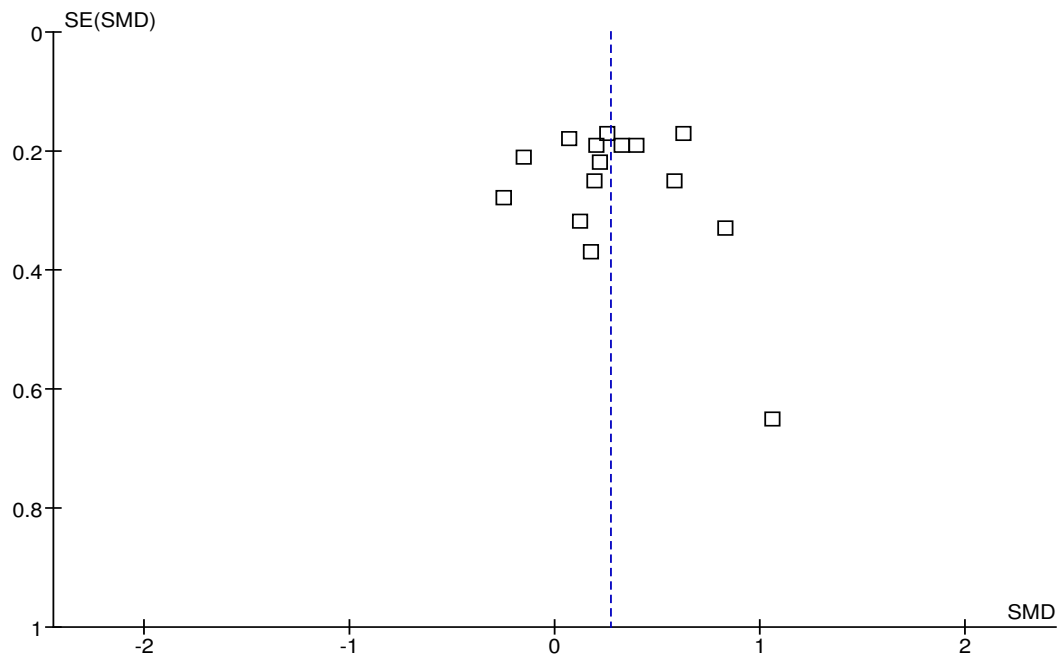
For high quality studies a medium effect size of 0.43 (95% CI 0.27, 0.60) was calculated which was significant ( $Z=5.15$ ,  $p<0.00001$ ). The test of heterogeneity indicated that there was a significant level of inconsistency across findings ( $p=0.002$ ). For low quality studies a medium effect size of 0.48 (95% CI 0.22, 0.74) was found which was significant ( $Z=3.62$ ,  $p<0.0003$ ). The test of heterogeneity indicated that there was a significant level of inconsistency across findings ( $p<0.0001$ ).

#### *6.4.2. Outcome measure*

The quality of life outcome measures could be classified as either general (e.g. SF-36 and Quality of Life Index) or disease-specific (e.g. Fibromyalgia Impact Questionnaire and Functional Assessment of HIV Infection). An independent-samples t-test indicated that there was not a significant difference in the mean effect sizes for general ( $M=0.41$ ,  $SD=0.35$ ,  $N=11$ ) and disease specific ( $M=0.66$ ,  $SD=0.44$ ,  $N=5$ ) quality of life measures ( $t=1.21$ ,  $p=0.25$ ).

### 6.5. Publication bias

This was assessed using a funnel plot of effect size against standard error for each study (Figure 6.3). This was calculated for the controlled study effect sizes only ( $N=14$ ). Subjectively, there appeared to be studies missing from the lower left-hand corner of the graph and this asymmetry was therefore suggestive of publication bias. The *fail-safe*  $N$  was calculated for the effect sizes in the controlled studies ( $k=14$ ). Analyses revealed that 79 unpublished studies with an effect size of zero would be required to make the population effect size non-significant. Therefore, according to Rosenthal (1993), because this figure does not exceed  $5k+10$ , the results cannot be considered robust to the effects of publication bias.



**Figure 6.3.** Funnel plot of standard error (SE) against effect size (SMD)

## **7. DISCUSSION**

### **7.1. Overview**

This section will examine the results in light of previous research. Strengths and limitations of the review will also be considered along with implications for clinical practice and future research.

### **7.2. Discussion of results in relation to the literature**

The aim of this review was to investigate the effect of MBSR on quality of life in people suffering from a chronic health condition. Nineteen studies were identified and the results indicated that MBSR had a small to moderate effect on quality of life when compared to a control condition (Hedges  $g=0.26$ ). When MBSR was compared to a waitlist control or treatment as usual condition only, the positive effect on quality of life was slightly larger ( $g=0.34$ ). However, a statistically significant effect size was not found when MBSR was compared to an active treatment control ( $g=0.16$ ). Within group effect sizes for the mindfulness groups were in the moderate range pre to post intervention ( $g=0.45$ ) and at follow-up ( $g=0.63$ ). Subgroup analysis indicated that effect sizes were not related to study quality or whether the outcome measure was generic or specific to a particular health condition.

There are three key aspects of the results which warrant further discussion. Firstly, the findings presented here indicate that there was not a significant effect on quality of life when MBSR was compared to an active treatment control. Secondly, effect sizes for quality of life were relatively small compared to the findings of other reviews. Thirdly, the effects of MBSR on quality of life appear to be maintained over time.

#### *7.2.1. MBSR compared to an active control*

The finding that MBSR does not have a significant effect on quality of life compared to an active control condition is somewhat consistent with other research. Other reviews investigating the effect of MBSR on different outcomes have found similar results. Bohlmeijer *et al.* (2010) summarised eight randomised controlled studies investigating effects of MBSR for distress in chronic medical conditions and reported

small to moderate between group effect sizes:  $g=0.26$  for depression;  $g=0.47$  for anxiety; and  $g=0.32$  for psychological distress. However, only one of their included studies, which was also included in the current review, compared MBSR to an active control. This was an education support group, and an almost negligible effect size of  $g=0.01$  for depression was reported for this study (Astin *et al.*, 2003).

Likewise, Chiesa and Serretti (2009) carried out a review of MBSR for a healthy population including 10 controlled studies. Only one study compared MBSR to an active control, which was relaxation training (Jain *et al.*, 2007), and this study found no significant difference between MBSR and relaxation training on the Brief Symptoms Inventory (BSI; Derogatis, 1992). Chiesa and Serretti conclude that the treatment specific effects of MBSR cannot be supported in light of this finding.

In the current review, five studies compared MBSR to an active control condition. However, these active control conditions were not based on validated and established interventions, with the exception of Gross *et al.* (2010) who compared MBSR to a health education group. This intervention was based on a published manual which has been investigated with successful results in a separate randomised controlled trial (Lorig *et al.*, 1999). The other four studies compared MBSR to a health education programme or support group which had not been previously investigated by independent researchers. Furthermore, checks for treatment integrity in either the mindfulness or active control groups were not carried out. Only one of these five studies reported intention to treat data (Mularski *et al.*, 2009) and therefore the overall effect size reported in the current review does fully not take into account the effects of drop out. Overall though, the quality of the five active control studies was reasonably good. All scored above the average quality score which was 13.3 and were ranked as being within the 10 highest quality studies included in this review.

Although the observed effect size comparing MBSR to an active control was small and non-significant, four of the five studies did find that MBSR was more effective than the active control for improving quality of life. Grossman *et al.* (2007) in particular found the largest effect size ( $g=0.83$ ) comparing MBSR to a support group.

This study was of average quality, scoring 14 out of 24, and did not complete intention to treat analysis. The control group was smaller than the mindfulness group (N=13 compared to N=39) and those in the mindfulness group had significantly higher pain severity than those in the control group pre-intervention. The greater effect size observed in this study may be, in part, explained by differences in symptom severity in the two groups pre intervention.

Mularski *et al.* (2009) on the other hand found that an active control was in fact more effective than MBSR for improving quality of life ( $g=-0.15$ ). This study was of slightly higher quality (scoring 17 out of 24) and the most striking aspect of this study in comparison to that of Grossman was the gender of participants. Grossman *et al.* (2007) investigated MBSR with an entirely female sample and Mularski *et al.* with an almost entirely male sample. This may suggest that MBSR is more effective for women than men. Male participants also seemed more likely to drop out of the mindfulness intervention. Mularski and colleagues reported an attrition rate of 50 per cent in the mindfulness group compared to only nine per cent in Grossman's female only study. The only other study investigating mindfulness with a primarily male sample was Robinson *et al.* (2003). A small effect size was also observed for this study ( $g=0.18$ ) and a high attrition rate of 35 per cent was reported for the MBSR group.

The active control studies in this review aimed to isolate the specific factors of MBSR. They did this by controlling for the 'non-specific' aspects of the mindfulness intervention such as social support, therapeutic alliance, group dynamics, psychoeducation and relaxation. The fact a significant effect size for these studies was not found may suggest it is not mindfulness *per se* which is effective but rather it is the aforementioned non-specific aspects which are responsible for positive outcomes.

One of the difficulties in disentangling the important factors of MBSR is that only two of the 19 studies included a measure of mindfulness pre and post intervention (Schmidt *et al.*, 2011; Witek-Janusek *et al.*, 2008). This may be because measures of

mindfulness are still being developed, but also researchers perhaps view this as an unnecessary step given their aim is to improve symptoms. However, it is not possible to attribute any observed changes on outcome measures such as quality of life to changes in mindfulness if a measure of mindfulness is not included. It is also not possible to determine whether an intervention is genuinely effective in increasing mindfulness if a measures of this construct is not included pre and post intervention. A similar criticism was made by Grossman *et al.* (2004) who noted in their review that mindfulness in many of the primary studies was neither operationalised nor evaluated.

In short, whilst the results point to the fact MBSR is not more effective than an active control group, there are factors which must be taken into consideration. Overall, study quality for the active control studies was reasonably good, though a weakness was the lack of intention to treat analyses and checks for treatment integrity. A small number of studies were used to calculate this overall effect size ( $N=5$ ) and only one compared MBSR to an established, previously validated intervention. Whilst it was not possible to reject the null hypothesis that the mean effect for MBSR compared to an active control is zero, effect sizes were generally in the direction indicating MBSR did have a small positive effect compared to an active control.

#### *7.2.2. Size of effect on quality of life*

The second aspect of the results which is striking is that MBSR had only relatively small effects on quality of life in a chronic health population relative to controls ( $g=0.26$ ). Previous meta-analyses which have investigated mindfulness training have generally found slightly larger effect sizes for other psychological outcomes such as anxiety, depression and psychological distress (Baer, 2003; Bohlmeijer *et al.*, 2010; Grossman *et al.*, 2004; Hofmann *et al.*, 2010).

Hofmann and colleagues (2010), for example, conducted a large scale review of 39 mindfulness-based interventions for a heterogeneous population including both psychiatric and medical conditions. Across this broad spectrum of participants,



mindfulness-based interventions had a moderate effect on depression ( $g=0.5$ ) and a large effect on anxiety ( $g=0.81$ ) when compared to an active treatment control.

Likewise, Grossman *et al.* (2004) reviewed 20 studies investigating MBSR for a range of people including stressed non-clinical groups, and those with psychological and medical conditions. MBSR had an effect of around  $g=0.5$  for mental and physical health outcomes respectively compared to a control group. Physical health outcomes included physical symptoms, pain, impairment and functioning; and mental health outcomes included psychological well-being, sleep, anxiety and depression.

In a more general review of psychosocial interventions for quality of life in cancer patients, Rehse and Pukrop (2003) calculated a standardised mean difference of  $d=0.65$  compared to controls across 37 published studies. Interestingly they found that interventions of at least 12 weeks were considerably more effective for increasing quality of life compared to under 12 weeks. MBSR in the current review was typically an eight week intervention and this could therefore be one explanation for the lower effect sizes observed.

Comparing the results of the current meta-analysis to those of previous reviews is made difficult by the wide ranging inclusion criteria and methodology. However, MBSR appears to have relatively small effects on quality of life in the weeks immediately following the intervention as compared to other outcomes and other psychological interventions. It may be that a ceiling effect is present whereby individuals included in this review did not have poor quality of life to begin with. For example, the baseline scores on the FAHI were higher than the average population in one of the included studies in this review (Robinson *et al.*, 2003). Carlson (2003) also report high ratings of quality of life prior to the intervention along with low levels of initial mood disturbance on the POMS.

Hofmann *et al.* (2010) found mindfulness-based therapies had a greater effect when studies specifically targeted populations with clinically significant levels of either

anxiety or depression. Bohlmeijer *et al.* (2010) also cite ceiling effects as a possible explanation for the small effect sizes observed in their review. The included studies in the current review did not have a cut-off for quality of life scores and as such the standard deviation of these scores will have been higher than in studies where a specific population was targeted. This may have led to smaller effect sizes and further analysis of studies including only participants with low quality of life scores at baseline may have been justified.

Another possible explanation for these results relates to the quality of life outcome measures themselves. Some measures require the individual to rate quality of life over a longer time period than commonly used measures of anxiety and depression. The Hospital Anxiety and Depression Scale (HADS; Zigmond & Snaith, 1983) is rated over a one week period as is the Beck Depression Inventory (BDI; Beck *et al.*, 1961). In contrast, the SF-36 requires the individual to rate most aspects of quality of life over the preceding four weeks. Given that mindfulness is typically an eight week intervention, when rating quality of life at post intervention participants are rating their quality of life as far back as only half-way into the intervention. There is a good argument for future studies investigating quality of life to administer questionnaires after a longer follow-up period to ensure positive changes are captured.

MBSR, as an intervention, is not aimed specifically at improving quality of life. As the name suggests, the main focus is on reducing stress. According to the founder, John Kabat-Zinn, MBSR can be applied in any context where “stress, emotional and physical pain, or illness and disease [are] primary concerns” (Kabat-Zinn, 2003, p. 148). The goal, however, of mindfulness training is not symptom reduction but rather to alter the *way* in which a person relates towards their internal experiences, particularly those that are negative (Grossman *et al.*, 2007).

The content of MBSR training may therefore target symptoms of anxiety and depression more so than aspects of quality of life, such as social functioning or physical health. It is possible that the small effect sizes observed in this review are due to the fact quality of life is a broad construct and MBSR affects the various

domains to different degrees. There are also aspects of quality of life, such as existential or spiritual well-being, which may be pertinent to quality of life for someone suffering from a life-threatening illness (Cohen *et al.*, 1996). MBSR may have larger effects on these specific domains which were not considered in the current review.

Given that mindfulness helps a person relate differently to their thoughts and feelings, it is reasonable to assume that MBSR might impact most on the psychological aspects of quality of life. Ledesma and Kumano (2009) reviewed MBSR as supportive therapy for cancer. They found effect sizes of  $d=0.37$  for mental health variables (including anxiety, depression, stress and psychological components of quality of life) compared to a control condition and  $d=0.17$  for physical health variables (including body mass, immune system function and physical symptoms).

Using the WHOQOL-BREF (WHOQOL Group, 1998), Nyklícek and Kuijpers (2008) compared MBSR to a WLC in a randomised study of a community sample with symptoms of distress. They found that MBSR improved quality of life relative to the WLC and that effect sizes were higher on the psychological domains of quality of life ( $d=0.21$ ) compared to the physical ( $d=0.19$ ) or social domains ( $d=0.13$ ). Furthermore, Nyklícek and Kuijpers found that quality of life as a whole was less sensitive to change following MBSR than psychological distress and positive affect.

In summary, whilst effect sizes for quality of life are relatively small compared to the effect on other aspects of well-being there are several possible explanations for this. A ceiling effect may have been present and it is possible that if studies had only included participants with poor quality of life to begin with, greater effects would have been observed. In addition, the nature of quality of life measures means they may be less sensitive to change immediately post intervention. Finally, the effect sizes must be considered in light of the fact MBSR is not specifically aimed at improving quality of life. It is likely that it impacts on different aspects of quality of life in different ways, and greater effect sizes may have been observed if the

psychological component of quality of life had been isolated. The fact that MBSR has small effects on overall quality of life, given these limitations, is really quite promising.

### 7.2.3. *Impact of MBSR over time*

The third aspect of the current results worth noting is that changes in quality of life appeared to be maintained at follow-up and that positive changes continued to take place post intervention. This was based on the finding that within group pre-follow-up effects ( $g=0.63$ ) were greater than those immediately post intervention ( $g=0.45$ ). However, the calculation of follow-up effect sizes was based on a subset of studies. Only nine of the overall 19 studies investigated quality of life after a follow-up period. In fact, of these nine, only two studies found that MBSR effects on quality of life were greater than they were immediately post intervention (Carlson *et al.*, 2007; Gross *et al.*, 2010). Grossman *et al.* (2007, p.229), for example, after a three year follow-up period, noted "a clear and substantial decline from post-intervention to follow-up" quality of life scores. However, scores at follow-up were still significantly improved from baseline. In all of the nine studies, effect sizes at follow-up were similar to those at post intervention, but not always greater.

Previous reviews have also found that the positive effects of mindfulness-based therapy are maintained at follow-up (e.g. Hofmann *et al.*, 2010; Baer, 2003). This may be because people who have had training in mindfulness seem to continue to practice mindfulness exercises post intervention. Kabat-Zinn *et al.* (1987) conducted what seems to be the longest term follow-up study of mindfulness meditation and found that 75 per cent of trainees continued to practice meditation over a follow-up period ranging from six to 48 months post intervention. Of this group 43 per cent practiced meditation three times per week or more, for 15 minutes or more at a time.

Similar results were found by Reibel *et al.* (2001) who noted that 70 per cent of participants after a one year follow-up indicated they practiced formal meditation five times per week for 10 to 20 minutes. Ninety-one per cent of participants claimed to practice informal meditation five times per week. Mindfulness practice may be an

important factor in maintaining or even enhancing gains observed immediately post intervention. Speca *et al.* (2000) measured the amount of home meditation practice following MBSR in a group of over 90 cancer outpatients. The time spent practicing at home predicted improvements in stress and mood disturbance. Likewise, Mathew *et al.* (2010) found that following MBCT for major depression, ongoing meditation practice was predictive of better outcome in the prevention of relapse.

So, although the current results suggest quality of life improves during a follow-up period, a more conservative interpretation is that gains are simply maintained. This finding is in line with previous reviews which have also found this to be the case. One reason may be that individuals who have had mindfulness training continue to use the techniques and skills learnt during the intervention. There is some evidence to suggest continued practice is a predictor of better outcome, immediately post intervention and further research is required to determine if this is also the case after a longer period post intervention.

It is also worth mentioning one final note about the quality of primary studies more generally. In general, the stronger studies in this review (Grossman *et al.*, 2010; Monti *et al.* 2006; Schmidt *et al.*, 2010) found that MBSR had a small to moderate effect size compared to a control condition ( $g$  ranging from 0.2 to 0.63) and a similar effect within group pre to post intervention ( $g$  ranging from 0.31 to 0.45). However, study quality was not found to be statistically associated with the size of within group pre-post effects. In fact, studies of higher methodological quality had comparable weighted pre-post effect sizes ( $g=0.43$ ) to those studies of lower methodological quality ( $g=0.48$ ). There was, however, a significant degree of heterogeneity amongst effect sizes of both groups. In the 'low' quality studies two of the four lowest quality studies had reasonably large effect sizes and standard errors (Bedard *et al.*, 2003; Surawy *et al.*, 2005). In contrast, the other two lowest quality studies had small effect sizes, one of which was in fact negative (Knauss, 2007). In previous reviews, generally studies of higher methodological quality have found smaller effects (e.g. Bohlmeijer *et al.*, 2010). The fact this was not the case in the current review could be related to the scale used to evaluate study quality.

Several factors which may have influenced effect sizes, such as how representative the sample was of the chronic condition in question, and the severity of the disorder, were not considered when rating study quality. It is also possible that further analysis of the results, such as meta-regression, would have been a more appropriate method to identify moderating variables, such as study quality. Meta-regression is an extension of subgroup analysis and The Cochrane Collaboration suggests it should only be used with caution and good rationale. Heterogeneity in effect sizes can be due to many factors, not always identified by further analysis (Higgins & Green, 2005). Given that heterogeneity was identified for the within group pre-post effects only, this was not considered necessary.

### **7.3. Strengths and limitations of the review**

In this review, although attempts were made to reduce publication bias by searching for unpublished studies and contacting first authors, there remains the potential that papers with weaker effects were less likely to have been included. Asymmetry in the funnel plot (Figure 6.3) was suggestive of publication bias and this was consistent with the calculation of Rosenthal's *fail-safe* N (Rosenthal, 1979).

Terrin *et al.* (2005) argue that funnel plots are a crude measure of publication bias because they rely on a subjective assessment of symmetry. They found that researchers identified moderate to high levels of visual asymmetry in funnel plots containing 10 studies even when no publication bias was present. Rosenthal's method has been criticised for being too dependent on whether or not the results of primary studies were significant (Field & Gillett, 2010). Furthermore, because it assumes that the mean effect for the unidentified, unpublished studies is zero, it does not take into account the possibility that these studies may have negative effect sizes. This would lead to an overestimation of the number of studies required to nullify effects (Borenstein *et al.*, 2009). Some meta-analyses in the field of mindfulness have also found evidence of publication bias (e.g. Hoffman *et al.* 2010). However, methods to estimate publication bias in some reviews have used less than 10 studies (e.g. Chiesa & Serretti, 2011) and this can lead to misinterpretation of asymmetry in funnel plots

(Terrin *et al.*, 2005). In short, publication bias in the current review cannot be ruled out and therefore estimates of effect size should be interpreted in view of this.

Bias during the study selection process was minimised by having a clearly defined set of inclusion and exclusion criteria established prior to undertaking the search, and by making the process of excluding studies explicit (see list of exclusions in Appendix 7). Study quality was assessed by two independent raters, as recommended by The Centre for Reviews and Dissemination (CRD, 2009), and scores compared in order to minimise errors and bias which might arise from only one researcher completing this task.

Although some limitations in the criteria used to assess study quality have already been discussed, these were considered to be a relative strength of this review. Previous reviews in this area have used less substantial and sometimes inappropriate scales. For example, a commonly used measure of study quality in other meta-analyses is the Jadad scale, a scale developed to assess the quality of randomised controlled trials specifically (Jadad *et al.*, 1996). Despite this, it has been used to evaluate controlled and uncontrolled studies alike (e.g. Hofmann *et al.*, 2010). Even when it is modified, as it was in Hoffman's review, this scale has four criteria out of five which are only relevant to controlled studies.

The CRD (2009) highlight the importance of having a clearly defined set of inclusion criteria, in terms of participants, intervention, comparators, outcomes and study design. The current review used a clear definition of 'quality of life' and only included studies which used outcome measures that adhered to this specific definition, something which is often neglected in the quality of life literature (Speight *et al.*, 2009). Studying exclusively MBSR as opposed to just 'mindfulness-based' therapies is a further strength as this improves the generalisability of the research. Other reviews of mindfulness have included interventions such as MBCT, ACT and non-specific 'mindfulness-based interventions'.

Whilst this review was very specific in terms of the intervention and outcome investigated, a heterogeneous sample of patients was included which is a weakness. Ten different chronic health conditions were present amongst the included studies and these various conditions are likely to differ significantly in their impact on quality of life. Arnold *et al.* (2004) for example found that physical and social functioning contributed to overall quality of life scores in only certain disorders such as lung disorder, hypertension and arthritis, but not others. Psychological functioning on the other hand contributed to overall quality of life in all disorders studied and in healthy subjects.

Power calculations in meta-analyses are rarely considered and according to some this is a substantial weakness in many published reviews. Muncer *et al.* (2002) argue that sufficient power in primary studies should be a criterion for inclusion in a meta-analysis, and that power analysis should be conducted for the meta-analysis as a whole. They highlight that it is often assumed in meta-analyses that power of individual studies is unimportant, given studies will be combined and thus power will be increased. However, power is not necessarily an additive property in a random effects meta-analysis (Cohn & Becker, 2003). Arguably, studies with inadequate power should have been excluded from this review. However, in an effort to capture as many studies as possible, in a field where research is still limited, these were included. Power was considered in the criteria used to assess methodological quality and therefore is reflected to some degree in this review.

#### **7.4. Implications for research and clinical practice**

Future research investigating MBSR in chronic health populations could be improved in several ways. Very few primary studies reported intention to treat analysis, which resulted in effect sizes being based exclusively on those who did not drop out of the MBSR or control intervention. This potentially leads to confounding of the results, and future studies should address this. On the other hand, this is exactly what happens in clinical practice and it is useful for clinicians to know the levels of improvement that they can expect among those patients who complete clinical interventions.



Controlling for therapist effects would also improve the research in this area which could be partially addressed by ensuring that more than one therapist delivers the control and mindfulness interventions. We know from many years of research that potentially there are countless factors (not just the mode of therapy) that can affect patient outcomes and that the "therapist effect" can be substantial (Beutler *et al.*, 2004). However, it is worth noting that the majority of therapeutic input in MBSR is self-delivered by participants who practice mindfulness by listening to tapes of about 30 to 40 minutes duration in between sessions. Furthermore, the nature of these tapes seems to vary little from one practitioner to another (Saunders, 2006).

The results presented here indicate MBSR does not have a statistically significant effective compared to an active control. However, various factors were identified which limit the interpretability of the current results. There is a need for future research to compare MBSR with other specific, established treatments, rather than treatment as usual or waitlist controls. In addition, research investigating MBSR would benefit from including a measure of mindfulness as a baseline and outcome measure. This would improve our understanding of the mechanisms of change and help to distinguish between the specific and non-specific effects of mindfulness interventions.

Due to the small number of studies, it was not possible in the current review to determine which specific health conditions MBSR is most effective for. Certainly the majority of studies investigated MBSR for fibromyalgia and chronic pain conditions, and the evidence base for this is encouraging. Future research could investigate more thoroughly MBSR for other conditions such as irritable bowel syndrome, HIV and heart disease. Future studies should also try to address the potential for ceiling effects by screening for participants with low quality of life at the start of the intervention.

Finally, there are clinical implications that arise from this review. The Department of Health notes that long term conditions require a significant amount of healthcare service resources. Those with long term conditions are the most frequent users of

healthcare services and it is estimated that half of all GP appointments and nearly three quarters of all inpatient bed days are used by this group (DoH, 2008). Given that long term conditions affect quality of life and that this can have implications for physical health outcomes and adherence, it is of great importance that healthcare services address this. Grumbach (2003, p.5) asserts that “the goals of care are to enhance functional status, minimize distressing symptoms, cope with the psychosocial stresses of pain and disability, and prolong life through secondary prevention.” The results of this review demonstrate that MBSR does have a role to play in improving the lives of individuals with chronic conditions and, being a group-based intervention of relatively short duration, is cost effective in doing so.

It is not clear, however, the extent to which the positive outcomes observed in this review are specific to MBSR. When MBSR was compared to an active control condition, a significant difference in quality of life effect sizes was not observed. This may suggest that it is the non-specific factors of MBSR which are contributing to positive outcomes. This has implications for clinical practice because delivering a mindfulness-based intervention depends on resources and training of therapists which can be costly, perhaps more so than, for example, a psychoeducation programme. In order to deliver mindfulness-based interventions, therapists must have had training and practice mindfulness meditation regularly themselves. Kabat-Zinn (2003, p.149) states: "mindfulness [...] cannot be taught to others in an authentic way without the instructor's practicing it in his or her own life." Further evidence to support the specific effects of MBSR are required before this use of therapist time and resources can be justified.

There is also the question of how generalisable the results of this review are. A wide range of health conditions were included, limiting the generalisability of the current findings to any one particular health condition. There was also a degree of heterogeneity in quality of life scores pre intervention and again it is not possible to determine which particular groups of individuals an MBSR intervention may be most effective for. The research suggests that the relationship between quality of life and chronic illness is not necessarily straightforward. In some studies, despite the

presence of a chronic condition, quality of life scores were similar to what would be expected in a healthy population. The extent to which MBSR will improve quality of life may depend on the extent to which quality of life is affected by the illness in the first place. It would be appropriate for clinicians to consider screening patients with a chronic illness for poor quality of life before considering an intervention to address this.

The studies included in this review had a much higher proportion of women and the results indicated that women were less likely to drop out of a mindfulness-based intervention. This may be an indication that MBSR is a more acceptable intervention to women and is a consideration for clinical practice. Future research could investigate gender differences in mindfulness, and particularly how mindfulness-based interventions could be adapted to be more acceptable to men.

The long term effects of MBSR in relation to quality of life found in this review are encouraging. There is some evidence to suggest that an important factor in whether or not long term gains are maintained is the extent to which home practice is continued post intervention. This may be an important consideration for clinicians delivering a mindfulness based intervention as the extent to which home practice is encouraged, monitored, and reflected upon during sessions may influence outcomes.

## **7.5. Conclusion**

This meta-analysis suggests that MBSR has a small to moderate effect on quality of life for individuals with chronic health conditions compared to controls. Given that MBSR is not specifically targeted towards improving quality of life, and that quality of life encompasses many aspects of well-being and functioning, this is encouraging. It would be unrealistic to expect large effect sizes because samples were not selected by setting a threshold to ensure relatively poor quality of life at baseline.

There has been a tendency over the past decade to consider mindfulness interventions as a “cure all” (Bishop *et al.*, 2002). Maybe this is because the goal of MBSR is broad in itself: simply learning to relate to internal experiences differently,

with an attitude of acceptance, is to some extent something we could all benefit from. As a consequence of this far-reaching goal, individuals learn skills which they can apply to almost any aspect of day to day life. The fact positive outcomes are maintained at follow-up and people continue to use these skills post-intervention supports this. This is perhaps one of the most promising aspects of MBSR as a therapeutic approach.

There is a prevalence of chronic health problems amongst the adult population and it is now accepted that the goal of healthcare is more than just improving or controlling physical symptoms. Psychosocial interventions also have a role to play in improving quality of life for these individuals, especially group-based interventions which are cost-efficient. MBSR is one such approach and this review demonstrates the positive impact it can have on quality of life.

## 8. JOURNAL ARTICLE

### MINDFULNESS-BASED STRESS REDUCTION AND QUALITY OF LIFE FOR PEOPLE WITH CHRONIC MEDICAL CONDITIONS: A META-ANALYSIS

J. V. Russell<sup>ab1</sup>, P. G. Morris<sup>a</sup>, A. J. A. Keen<sup>a</sup> and D. Peck<sup>a</sup>

<sup>a</sup>*University of Edinburgh*

<sup>b</sup>*NHS Grampian*

**Purpose:** The aim of this review was to investigate the efficacy of Mindfulness-Based Stress Reduction (MBSR) for improving quality of life in individuals with chronic health conditions.

**Methods:** A literature search was conducted in February 2011 using the following databases: AMED; PsycINFO; MEDLINE; and EMBASE. Controlled and observational studies were included and the methodological quality of each was assessed using specific criteria. Effect sizes were calculated using Hedges'  $g$  and a random effects meta-analysis was conducted.

**Results:** The search identified 19 studies which met the inclusion criteria. These included a total of 1424 participants with a range of chronic conditions. MBSR led to greater improvements in quality of life compared to a control condition and the effect size was small to moderate (Hedges  $g=0.26$ ). Within group effect sizes were also calculated indicating that MBSR had a moderate effect pre to post treatment (Hedges  $g=0.45$ ) and at follow-up (Hedges  $g=0.63$ ).

**Conclusion:** These findings suggest that MBSR does have a positive and significant effect on quality of life for individuals with chronic health problems. These effects appear to be maintained at follow-up. However, publication bias and ceiling effects may have been present and there was some evidence to suggest that MBSR was not more effective than active treatment controls.

**Keywords:** Mindfulness, quality of life, chronic condition, physical health, meta-analysis, mindfulness-based stress reduction, MBSR

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<sup>1</sup> Address for correspondence: Jane V. Russell, Clinical and Health Psychology, School of Health in Social Science, University of Edinburgh, Edinburgh, EH8 9AG. Email: janevrussell@gmail.com

## Introduction

Chronic physical health conditions, such as cardiovascular disease, cancer and diabetes, are the leading cause of death and disability worldwide (WHO, 2008). In 2001, it was estimated that there were up to 17.5 million adults living with a chronic disease in the UK alone (Department of Health, 2001). Chronic conditions, by their very nature, are enduring, have a pattern of recurrence or deterioration, and have a poor prognosis. Increasingly, it is recognised that health care provision for these individuals is as much about improving quality of life as it is about continuous and complex management of symptoms.

The World Health Organisation (WHO) defines health as “a state of complete physical, mental and social well-being and not merely the absence of disease and infirmity” (WHO, 1947). Studies indicate that chronic health problems impact significantly on all of these aspects of well-being. Regarding the physical impact, a recent review of the literature concluded that physical function was “severely and negatively affected” by chronic disease (Hopman *et al.*, 2009, p.108). In their study of 1574 patients Bayliss *et al.* (2004) found that congestive heart failure, diabetes and chronic respiratory disease predicted a clinically significant decline in physical functioning, as did the presence of multiple co-morbid chronic conditions.

Perhaps less obvious are the effects of chronic conditions on social well-being and mental health. Although mental health seems to be the domain least likely to be affected by chronic illness (Hopman *et al.*, 2009; Stewart *et al.*, 1989; van't Spijker *et al.*, 1997) it is now well established through research that people suffering from chronic health problems are at a greater risk of psychological distress. The probability of having depression, for example, is significantly higher in the presence of a chronic health condition (Egede, 2007; Moussavi *et al.*, 2007). NICE (2009) report that around 15-25% of people with conditions such as coronary heart disease, diabetes, cancer and stroke met the diagnostic criteria for depression and that depression was about two to three times more common in these patients than in people without physical health problems. Mental health has also been found to impact on adherence to treatment and health behaviour recommended for the physical condition (Prince *et al.*, 2007; Sabaté, 2003).

Chronic illness can also affect social functioning by impacting on aspects such as marital relationships, parental responsibilities and social activities (Hahn *et al.*, 2010; Verhaak *et al.*, 2005). Michael *et al.* (2000) compared the functional health status of over 700 women diagnosed with breast carcinoma to healthy controls and found that a decline in both physical and social function was present up to four years post-diagnosis. In a large scale study, Stewart *et al.* (1989) found that chronic conditions had a marked negative impact on physical, role and social functioning, with myocardial infarction and congestive heart failure impacting the most on social functioning.

The fact chronic illness can affect so many aspects of well-being has had implications for outcome research in this area. Outcome measures such as quality of life have commonly been used as markers for change following medical and psychological interventions in this population. Although a widely used term, 'quality of life' remains an ambiguous concept. In common with most definitions is the fact quality of life is multidimensional, subjective, and dynamic (i.e. changing over time) including aspects of an individual's physical, functional, emotional and social well-being (Cella, 1994; Speight *et al.*, 2009). Perhaps because it is multidimensional, it has been widely used as an outcome measure to evaluate interventions for people with chronic health problems.

A distinction has been made between quality of life and health-related quality of life; the latter encompassing an individual's perception of the impact of their illness and its treatment(s) (Speight *et al.*, 2009). Consequently, health-related quality of life has been developed to describe aspects of an individual's subjective experience that relate both directly and indirectly to health, disease, disability, impairment and to the effectiveness of treatment (Carr *et al.*, 2001). In practice, these terms are used interchangeably and particularly in chronic health populations, Moons *et al.* (2006) argue that a distinction is irrelevant as it would be difficult, if not impossible, for a person to distinguish between parts of their life influenced by their health or not.

It is recognised that chronic physical health conditions impact significantly on an individual's quality of life and increasingly psychological interventions are being considered to address this. One intervention which is increasingly being

applied in chronic health settings to improve quality of life is Mindfulness-Based Stress Reduction (MBSR). This was originally developed for patients with chronic pain and stress-related disorders (Kabat-Zinn, 1990). Based on the principles of mindfulness meditation, it encourages participants to focus their attention on the present moment and adopt an attitude of acceptance and openness to that experience, be it positive or negative. One of the most distinguishing features of mindfulness as a psychological intervention is the emphasis on acceptance of negative cognitive or physical experiences, rather than seeking to change or distract from them. This attitude of acceptance may be particularly important for individuals whose chronic health problems cause a host of negative psychological and physical symptoms (Reibel *et al.*, 2001).

Empirical research of mindfulness-based therapies is growing and several meta-analyses published in the last five years show promising results. Most recently, Hofmann *et al.* (2010) investigated the effect of mindfulness-based therapies (i.e. not exclusively MBSR) in a heterogeneous sample of people suffering from clinical levels of anxiety or depression. A moderate effect sizes from 39 studies was found for symptoms of anxiety (Hedges'  $g=0.63$ ) and depression (Hedges'  $g=0.59$ ). Bohlmeijer *et al.* (2010) found small to moderate effect sizes for anxiety, depression and psychological distress in adults with chronic medical diseases across eight controlled studies. Similar findings are reported for the effect of MBSR for reducing anxiety and depression in cancer patients (Ledesma & Kumano, 2009) and reducing stress in a healthy population (Chiesa & Serretti, 2009).

It seems clear from the evidence that mindfulness has small to moderate effects on reducing psychological distress in a wide range of populations. However, it is not clear what effect it has on other aspects of quality of life which we know are affected by chronic illness. In particular, the current reviews do not address the extent to which MBSR improves the positive aspects of mental health, and physical and social functioning. The purpose of the current meta-analysis therefore was to determine the efficacy of MBSR for improving quality of life in people with chronic health problems. Quality of life was chosen as an outcome measure because it was considered to better reflect the impact of chronic illness than simply the absence of psychological distress.



## **Method**

This review was conducted in accordance with the QUOROM statement (Moher *et al.*, 1999) which is included in Appendix 8.

### ***Inclusion and exclusion criteria***

#### ***Participants***

Studies were included if participants were adults over the age of 18 with a chronic health condition. The term ‘chronic condition’ was defined by O'Halloran *et al.* (2004, p.384) as a condition which has:

- a) A duration that has lasted, or is expected to last, at least 6 months
- b) A pattern of recurrence or deterioration
- c) A poor prognosis
- d) Consequences or sequelae that impact on the individual's quality of life.

This study focussed only on those conditions which met all of these criteria and were considered to be physical health conditions only. Bohlmeijer *et al.* (2010, p.540) define physical health conditions as “*any conditions which involve some disability, caused by irreversible pathological change... [including]... illnesses that need not be irreversible but cause enduring disability (e.g., cancer).*” Studies were therefore included if participants had a diagnosable physical health condition (as described by Bohlmeijer *et al.*, 2010) which was considered to be chronic, based on O'Halloran's four criteria.

#### ***Intervention***

Studies were included if they involved Mindfulness-Based Stress Reduction (MBSR), the programme developed by Kabat-Zinn and colleagues (Kabat-Zinn, 1990). The MBSR had to be delivered as weekly group therapy of at least 6 weeks duration. Studies where the intervention was ‘based on’ MBSR were also included so long as the previous two conditions were met (i.e. group therapy and at least 6 weeks duration) and where the predominant therapeutic approach was MBSR.

### *Outcomes*

This meta-analysis examined studies where quality of life was used as an outcome measure. There is no agreed definition of quality of life but there is broad agreement in the literature that it is a multidimensional construct encompassing the subjective evaluation of the physical, psychological and social aspects of one's life (Bowling, 2005; Speight *et al.*, 2009). Therefore, studies were included if they utilised a self-report measure, pre and post intervention, which assessed quality of life in terms of all three of these domains. Single item measures, such as visual analogue scales, were excluded as they do not sufficiently assess the multi-dimensional nature of quality of life (Cella, 1994).

### *Study types*

Only quantitative studies published in English were considered for this review. Both controlled and observational trials were included if they published sufficient data to allow for the calculation of an effect size (e.g. means, standards deviations or test statistics).

### *Search strategy*

The search strategy was designed to identify all studies investigating MBSR as an intervention for people with chronic health conditions. The search was conducted in February 2011 by the first author. The following databases were searched: AMED (1985-Feb 2011); EMBASE (1947-Feb 2011); MEDLINE (1950-Feb 2011); and PsycINFO (1806-Feb 2011) using OVIDSP. The search terms used were: *mindful\**, *MBSR*, *meditation*, *stress reduction*, *chronic*, *physical*, *medical health*, *disease*, *condition*, *quality of life*, *well-being*, and *life satisfaction*. These terms were searched within the domains of title, abstract and key words.

The contents pages of the following journals were searched for relevant articles using a Boolean search with the term *mindful\** anywhere in the article titles: *Journal of Alternative and Complimentary Medicine* (1998 to 2011); *Journal of Psychosomatic Research* (1960 to 2011); *British Journal of Health Psychology* 1998-2006; *Psychosomatic Medicine* (1939 to 2011) and relevant articles were screened based on the title and abstract. The reference lists of included studies were also

screened for relevant articles and first authors contacted in order to identify any unpublished research.

### ***Assessment of study quality***

Each study was coded for methodological quality using a set of criteria which were developed specifically for this review. Guidance given by The Centre for Reviews and Dissemination (CRD, 2009), SIGN guidelines (SIGN, 2008), and scales used by other authors for similar reviews were considered in the development of these criteria (e.g. Ost, 2008; Hofmann *et al.*, 2010; Cartwright-Hatton *et al.*, 2004). Studies were rated using 2 points for 'well addressed'; 1 point for 'adequately addressed'; and 0 points for 'poorly addressed', 'not applicable', or 'not reported'. All studies were coded by the first author and a random sample of 9 studies was coded by the second author. Ratings were compared for consistency between authors and amendments made where appropriate.

The criteria were as follows:-

- a) Control: Treatment condition was compared to that of a suitable control (e.g. treatment as usual (TAU), wait-list control (WLC) or another empirically documented treatment).
- b) Group similarities: Patient characteristics (age, sex, disease status, quality of life pre-intervention scores) were similar in the treatment and control condition prior to the intervention and if not this difference was controlled for statistically.
- c) Allocation: Patients were allocated randomly to groups.
- d) Diagnosis was confirmed by a trained clinician prior to enrolment in the study.
- e) Attrition rate was 10% or less in each group(s).
- f) Appropriate intention to treat analysis was used.
- g) A power calculation was reported and sample size was decided and obtained accordingly.
- h) Treatment was delivered by suitably trained, experienced therapists.
- i) Treatment was manualised, replicable and specific.

- j) Efforts were undertaken to minimise individual therapist effects.
- k) Quality of life outcome measure was deemed to be valid and reliable, and Cronbach's  $\alpha \geq 0.7$ .
- l) Outcome measurement took place pre and post intervention, and at least 3 months after the intervention was complete.

### ***Statistical analysis***

The standardised mean difference was calculated for each study using Hedge's  $g$ , a version of Cohen's  $d$  adjusted to minimise the effect of bias from small sample size. Both within group and between group effect sizes were calculated for continuous measures of quality of life. These were calculated in Microsoft Excel using the formulas described in Appendix 6. The magnitude of Hedges'  $g$  was interpreted using the convention: small (0.2), medium (0.5), and large (0.8) (Cohen, 1988).

The correlation between pre and post values for within group differences was required in order to calculate effect sizes. As this information was not reported in studies, a conservative estimate of  $r=0.7$  was used for all calculations (Hofmann *et al.*, 2010; Grossman *et al.*, 2004).

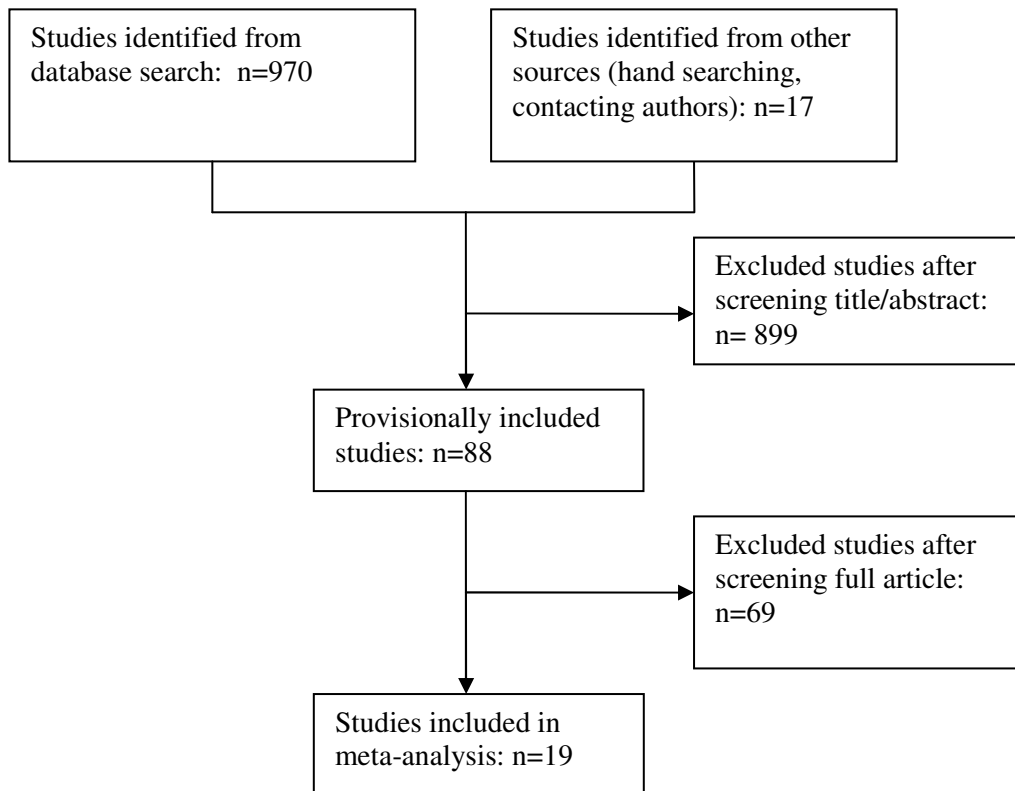
A random effects meta-analysis was conducted using the Cochrane Collaboration Review Manager Software (RevMan version 5.0). This calculates the weighted mean of effect sizes, where the effect size in each study is weighted by the inverse of its variance. The null hypothesis that the mean effect is zero was tested and the data assessed for heterogeneity.

Meta-analyses may be at risk of overestimating overall effect sizes because studies with insignificant results are less likely to be published. This publication bias has been termed the 'file drawer problem' (Rosenthal, 1979) and was assessed in the current review by calculating the *fail safe N* (see Appendix 6 for details).

## **Results**

### ***Overview of included studies***

A total of 987 articles were identified from the search process described, of which 19 met the inclusion criteria and were included in the meta-analysis. The flow chart in Figure 1 describes the process of identifying studies.



**Figure 1. Flowchart illustrating literature search process**

The included studies are summarised in Table 1. Collectively, these studies investigated mindfulness with 1424 participants (72% female), with a mean age of 51.8 (SD=9.38). Studies were primarily controlled trials (74%), and used a waitlist control (WLC), treatment as usual (TAU), or an active control (AC) condition. The most common disorder studied was chronic pain or fibromyalgia, followed by cancer. Ten studies measured quality of life after a follow-up period, which ranged from 1 month to 3 years post intervention (M=39.9 weeks, SD=44.9).

### ***Study quality***

The first two authors independently assigned the same methodological quality score for 84/101 (83%) of ratings, and differed by 1 point on 15/101 15% and by 2 points on 2/101 (2%) of ratings. Where differences between ratings of the first and second author occurred, these were discussed and changes made where it was deemed appropriate to do so. The average quality score was 13.3 (SD 5.6), out of a possible 24 points.

## ***Quantitative data synthesis***

### ***Between group effect sizes***

Fourteen studies compared MBSR to a control condition. One study compared MBSR to two different control conditions (WLC and AC) and therefore a conservative approach was taken to only include the AC data in this part of the analysis (Schmidt *et al.*, 2011).

Controlled effect sizes were also analysed in two separate groups: those using an active treatment control (N=5) and those using a WLC or TAU (N=10)<sup>1</sup>. The decision to combine WLC with TAU was based on the rationale that people tend to continue receiving other treatment whilst in a wait list control condition, which would therefore be similar to TAU. All effect sizes refer to the standardised mean difference of quality of life scores between the control and treatment group.

Across all controlled studies, MBSR led to greater improvements in quality of life compared to a control condition. A small effect size of 0.26 (95% CI 0.11, 0.41) was found which was statistically significant ( $Z=3.33$ ,  $p=0.0009$ ). The test of heterogeneity was not significant ( $p=0.12$ ) indicating that the effect sizes were consistent across studies and that further subgroup analysis was not appropriate.

Five studies compared MBSR to an active treatment control and although MBSR did lead to greater effects on quality of life compared to the active control, the effect was small and insignificant. An effect size of 0.16 (95% CI -0.09, 0.41) was found, which was not statistically significant ( $Z=1.28$ ,  $p=0.24$ ). The test of heterogeneity was not significant ( $p=0.16$ ).

Ten studies compared MBSR with WLC or TAU conditions and a small effect size of 0.34 (95% CI 0.18, 0.50) was calculated, which was highly statistically significant ( $Z=4.18$ ,  $p=0.0001$ ). This indicated that MBSR led to greater improvements in quality of life than either a WLC or TAU condition. The test of heterogeneity was not significant ( $p=0.29$ ) indicating that further subgroup analysis was not appropriate.

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<sup>1</sup> In this case both sets of data from Schmidt and colleagues was entered, resulting in a total N=15 rather than N=14 for this section.

### *Within group effect sizes*

Data to calculate within group effect sizes was available from 18 out of the 19 studies. One study did not report sufficient pre-post data to calculate an effect size (Witek-Janusek *et al.*, 2008). Effect sizes were calculated for all individuals who received a mindfulness intervention (N=817). This included isolating the mindfulness group data from controlled studies and calculating the pre-post effect sizes for the treatment group only. A medium effect size of 0.45 (95% CI 0.31, 0.59) was calculated, which was highly statistically significant ( $Z=6.24$ ,  $p<0.00001$ ). The test of heterogeneity indicated that there was a significant level of inconsistency across findings ( $p<0.00001$ ).

### *Within group effect sizes after follow-up*

Data was available for nine studies which investigated the effect of MBSR after a follow-up period. These ranged from one month to three years after the end of the intervention. A medium effect size of 0.63 (95% CI 0.41, 0.85) was calculated which was highly significant ( $Z=5.62$ ,  $p<0.00001$ ). The test of heterogeneity indicated that there was not a significant level of inconsistency across findings ( $p=0.14$ ).

### *Study quality*

A Pearson product-moment correlation was computed to assess the relationship between study quality and within group pre-post effect sizes, for which there was found to be heterogeneity. This was not significant ( $r=-0.02$ ,  $n=18$ ,  $p=0.947$ ).

### *Publication bias*

The *fail-safe* N was calculated for the effect sizes from the controlled studies ( $k=14$ ). Analyses revealed that 79 unpublished studies with an effect size of zero would be required to make the population effect size non-significant. Therefore, according to Rosenthal (1991), because this figure does not exceed  $5k+10$ , the results cannot be considered robust to the effects of publication bias.

**Table 1. Overview of included studies**

Author (reference)	Medical Condition	Groups	N	Mean age in years (sd)	QoL measure	Follow up (in weeks)	Study quality score	Effect size <i>g</i> (within group pre-post)	Effect size <i>g</i> (within group pre-follow up)	Effect size <i>g</i> (between group differences)
Astin <i>et al.</i> (2003)	Fibromyalgia	MBSR Education group	32 33	47.7 (10.6)	FIQ	24	16	0.63	0.61	0.19
Bedard <i>et al.</i> (2003, 2005)	Brain injury	MBSR Drop-outs	10 3	43 39	SF-36	52	6	1.06	0.86	1.06
Carlson <i>et al.</i> (2003, 2007)	Cancer	MBSR	59	54.5 (10.9)	EORTC QLQ-C30	52	7	0.26	0.29	n/a
Cusens <i>et al.</i> (2010)	Chronic pain	MBPM TAU	32 20	46.7 (11.5) 48.4 (12.3)	SF-36	none	15	0.30	n/a	-0.25
Gardner-Nix <i>et al.</i> (2008)	Chronic pain	MBCPM WLC	99 57	51 52	SF-36	none	6	0.24	n/a	0.26
Gross <i>et al.</i> (2010)	Solid organ transplant recipients	MBSR Health ed group	63 59	55 (11.3) 52 (10.4)	SF-12	52	18	0.74	0.84	0.07
Grossman <i>et al.</i> (2007)	Fibromyalgia	MBSR AC	39 13	54.4 (8.3) 48.8 (9.1)	QOL	156	14	0.92	0.61	0.83
Grossman <i>et al.</i> (2010)	Multiple Sclerosis	MBI TAU	76 74	45.9 (10.0) 48.7 (10.6)	QOL HAQUAMS	none	23	0.37	n/a	0.63
Knauss (2007)	Cancer	MBSR	20	Range 31-70	FACT-G	none	5	-0.05	n/a	n/a
Lengacher <i>et al.</i> (2009)	Cancer	MBSR TAU	40 42	57.5 (9.4)	SF-36	none	17	0.38	n/a	0.22
Ljótsson <i>et al.</i> (2010)	IBS	MBSR	34	34.6 (11.0)	IBS-QOL	26	13	1.37	1.3	n/a
Monti <i>et al.</i> (2006)	Cancer	MBAT WLC	56 55	53.1 (12.4) 54.1 (10.7)	SF-36	none	20	0.31	n/a	0.33
Morone <i>et al.</i> (2008)	Chronic pain	MBSR WLC	19 18	74.1 (6.1) 75.6 (5.0)	SF-36	13	17	0.33	0.31	0.12
Mularski <i>et al.</i> (2009)	Chronic obstructive lung disease	MBBT Support group	44 42	70.6 (10.6) 64.0 (9.1)	Veterans SF-36	none	17	-0.06	n/a	-0.15



Author (reference)	Medical Condition	Groups	N	Mean age in years (sd)	QoL measure	Follow up (in weeks)	Study quality score	Effect size <i>g</i> (within group pre-post)	Effect size <i>g</i> (within group pre-follow up)	Effect size <i>g</i> (between group differences)
Robinson <i>et al.</i> (2003)	HIV	MBSR TAU	23 10	43.1 (6.1) 36.1 (8.0)	FAHI	none	11	0.32	n/a	0.18
Rosenzweig <i>et al.</i> (2010)	Chronic pain	MBSR	99	49.8	SF-36	none	8	0.36	n/a	n/a
Schmidt <i>et al.</i> (2011)	Fibromyalgia	MBSR AC WLC	53 56 59	53.4 (8.7) 51.9 (9.2) 52.3 (10.9)	QOL FIQ	8	20	0.45	0.36	0.2(AC) 0.4(WLC)
Surawy <i>et al.</i> (2005)	Chronic fatigue syndrome	MBSR (1) MBSR (2)	10 9	18-65	FIS	12	6	0.73	0.67	n/a
Witek-Janusek <i>et al.</i> (2008)	Breast cancer	MBSR WLC	38 28	55 (10) 54 (8)	QLI	4	13	-	n/a	0.58

EORTC QLQ-C30: European Organisation for Research and Treatment of Cancer Quality of Life Questionnaire; FACT-G: Functional Assessment of Chronic Illness Therapy – General; FAHI: Functional Assessment of HIV Infection; FIQ: Fibromyalgia Impact Questionnaire; FIS: Fatigue Impact Scale; HAQUAMS: Hamburg Quality of Life Questionnaire in Multiple Sclerosis; IBS-QOL: Irritable Bowel Syndrome – Quality of Life Measure; QLI: Quality of Life Index; QOL: Profile of Health-Related Quality of Life in Chronic Disorders; SF-12: Short Form-12 Health Survey; SF-36: Short Form-36 Health Survey; Veterans SF-36: Veterans Short Form-36

AC: active control; MBPM: Mindfulness-Based Pain Management; MBCPM: Mindfulness-Based Chronic Pain Management; MBI: Mindfulness-Based Intervention; MBAT: Mindfulness-Based Art Therapy; MBBT: Mindfulness-Based Breathing Therapy; TAU: treatment as usual; WLC: waitlist control

## Discussion

The aim of this review was to investigate the effect of MBSR on quality of life in people suffering from a chronic health condition. Nineteen controlled and observational studies were identified and the results indicated that MBSR had a small to moderate effect on quality of life when compared to a control condition (Hedges  $g=0.26$ ). When MBSR was compared to a waitlist control or treatment as usual condition only, the positive effect on quality of life was slightly larger ( $g=0.34$ ). However, a statistically significant effect size was not found when MBSR was compared to an active treatment control ( $g=0.16$ ). Within group effect sizes for the mindfulness groups were in the moderate range pre to post intervention ( $g=0.45$ ) and at follow-up ( $g=0.63$ ). Further analysis indicated that effect sizes were not associated with study quality.

The finding that MBSR does not have a significant effect on quality of life compared to an active control condition is somewhat consistent with other research (e.g. Bohlmeijer *et al.*, 2010; Chiesa & Serretti, 2009). In the current review, only five studies compared MBSR to an active control condition but these were not validated and established interventions, with the exception of Gross *et al.* (2010). The other four studies compared MBSR to a health education programme or support group which had not been previously investigated by an independent group. Only one of these five studies reported intention to treat data (Mularski *et al.*, 2009) and therefore the overall effect size reported in the current review does not fully take into account the effects of drop out.

The active control studies in this review aimed to isolate the specific factors of MBSR. They did this by controlling for the 'non-specific' aspects of the mindfulness intervention such as social support, therapeutic alliance, group dynamics, psychoeducation and relaxation. The fact a significant effect size for these studies was not found may suggest it is not mindfulness *per se* which is effective but rather it is the aforementioned non-specific aspects which are responsible for positive outcomes. Furthermore, because only two of the 19 studies included a measure of mindfulness pre and post intervention (Schmidt *et al.*, 2011; Witek-Janusek *et al.*, 2008) it is not possible to know if MBSR increased mindfulness or if this was mediating positive outcomes.

Previous meta-analyses of mindfulness have generally found slightly larger effects for psychological outcomes such as anxiety, depression and psychological distress (Baer, 2003; Bohlmeijer *et al.*, 2010; Grossman *et al.*, 2004; Hofmann *et al.*, 2010). It may be that a ceiling effect was present whereby individuals included in the current review did not have poor quality of life to begin with. The baseline quality of life scores were higher than the average population in one of the included studies (Robinson *et al.*, 2003), and Carlson (2003) also report high ratings of quality of life prior to the intervention along with low levels of initial mood disturbance. Hofmann *et al.* (2010) found mindfulness-based therapies had a greater effect when studies specifically targeted populations with clinically significant levels of either anxiety or depression.

MBSR, as an intervention, is not aimed specifically at improving quality of life. The goal is not symptom reduction but rather to alter the way in which a person relates towards their internal experiences, particularly those that are negative (Grossman *et al.*, 2007). The content of MBSR training may therefore target symptoms of anxiety and depression more so than aspects of quality of life, such as social functioning or physical health. It is possible that the small effect sizes observed in this review are due to the fact quality of life is a broad construct and MBSR affects the various domains to different degrees. There are also aspects of quality of life not considered in this review. Existential or spiritual well-being, which may be pertinent to quality of life for someone suffering from a life-threatening illness, may have been influenced by MBSR (Cohen *et al.*, 1996).

Positive changes in quality of life appeared to increase after a follow-up period post intervention. However, the calculation of follow-up effect sizes was based on a subset of nine studies and of these, only two in fact found that effects on quality of life were greater than at post intervention (Carlson *et al.*, 2007; Gross *et al.*, 2010). In all of the nine studies, effect sizes at follow-up were similar to those at post intervention, but not always greater.

Previous reviews have also found that the positive effects of mindfulness-based therapy are maintained at follow-up (e.g. Hofmann *et al.*, 2010; Baer, 2003). This may be because people who have had training in mindfulness seem to continue to practice mindfulness (Kabat-Zinn *et al.*, 1987). Reibel *et al.* (2001) noted that 70%

of participants at 1-year follow-up indicated they practiced formal meditation five times per week for 10-20 minutes and 91% claimed to practice informal meditation five times per week. Mindfulness practice may be an important factor in maintaining or even enhancing gains observed immediately post intervention. Speca *et al.* (2000) measured the amount of home meditation practice following MBSR in a group of over 90 cancer outpatients and time spent practicing at home predicted improvements in stress and mood disturbance post-intervention.

In general, the studies of stronger methodological quality (Grossman *et al.*, 2010; Monti *et al.* 2006; Schmidt *et al.*, 2010) found that MBSR had a small to moderate effect size compared to a control condition and similarly within group pre to post intervention. However, study quality was not found to be statistically associated with the size of within group pre-post effects. This was probably due to the fact studies of low quality found quite different effect sizes. Two of the four lowest quality studies had reasonably large effect sizes and standard errors (Bedard *et al.*, 2003; Surawy *et al.*, 2005). In contrast, the other two low quality studies had small effect sizes, one of which was in fact negative (Knauss, 2007). In previous reviews, generally studies of higher methodological quality have found smaller effects (e.g. Bohlmeijer *et al.*, 2010). The fact this was not the case in the current review could be related to the scale used to evaluate study quality. Several factors which may have influenced effect sizes, such as how representative the sample was of the chronic condition in question or the severity of the disorder, were not considered when rating study quality.

### ***Strengths and limitations of the review***

Although attempts were made to reduce publication bias, this could not be ruled out, and there remains the potential that papers with weaker effects were not included. Study quality was assessed by two independent authors and scores compared in order to minimise errors and bias which might otherwise arise. Whilst this review was specific in terms of the intervention and outcome investigated, a heterogeneous sample of patients was included which is a weakness. Ten different chronic health conditions were present amongst the included studies and these various conditions are likely to differ significantly in their impact on quality of life.

### ***Implications for research and clinical practice***

Future research investigating MBSR in chronic health populations could be improved in several ways. Studies should report intention to treat analysis, particularly as dropout rates were quite high in some of the primary studies. Research could focus on comparing MBSR to other specific, established treatments, rather than treatment as usual or waitlist controls. This would help to shed light on the specificity of mindfulness-based interventions. In addition, studies should include a measure of mindfulness as a baseline and outcome measure. Future studies should also try to address the potential for ceiling effects by screening for participants with low quality of life prior to the intervention.

Finally, there are the clinical implications that arise from this review. The Department of Health highlights that long term conditions require a significant amount of healthcare service resources. Those with long term conditions are the most frequent users of healthcare services and it is estimated that half of all GP appointments and nearly three quarters of all inpatient bed days are used by this group (DoH, 2008). Given that long term conditions affect quality of life and that this can have implications for physical health outcomes and adherence, it is important that healthcare services address this. According to Grumbach (2003, p.5), “the goals of care are to enhance functional status, minimize distressing symptoms, cope with the psychosocial stresses of pain and disability, and prolong life through secondary prevention.” The results of this review demonstrate that MBSR does have a role to play in improving the lives of individuals with chronic conditions and, being a group-based intervention of relatively short duration, is cost effective in doing so.

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### **Declaration of Interest**

None.

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Williams, A.L., Selwyn, P.A., Liberti, L., Molde, S., Njike, V.Y., McCorkle, R., *et al.* (2005). A randomized controlled trial of meditation and massage effects on quality of life in people with late-stage disease: A pilot study. *Journal of Palliative Medicine*, 8(5), 939-52.

Wong, S.Y.S. (2009). Effect of mindfulness-based stress reduction programme on pain and quality of life in chronic pain patients: A randomised controlled clinical trial. *Hong Kong Medical Journal*, 15, 13-4.

Young, R.P. (1999). *The experiences of cancer patients practicing mindfulness meditation*. Unpublished thesis, Saybrook Institute, CA, USA.

Zautra, A.J., Davis, M.C., Reich, J.W., Nicassario, P., Tennen, H., Finan, P., *et al.* (2008). Comparison of cognitive behavioral and mindfulness meditation interventions on adaptation to rheumatoid arthritis for patients with and without history of recurrent depression. *Journal of Consulting and Clinical Psychology*, 76(3), 408-421.



## Appendix 1

### Guidelines for submission to Clinical Psychology Review



## CLINICAL PSYCHOLOGY REVIEW

### AUTHOR INFORMATION PACK

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ISSN: 0272-7358

#### DESCRIPTION

*Clinical Psychology Review* publishes substantive reviews of topics germane to clinical psychology. Its purpose is to help clinical psychologists keep up-to-date on relevant issues outside of their immediate areas of expertise by publishing scholarly but readable reviews. Papers cover diverse issues including: psychopathology, psychotherapy, behavior therapy, behavioral medicine, community mental health, assessment, and child development.

Reviews on other topics, such as psychophysiology, learning therapy, and social psychology, often appear if they have a clear relationship to research or practice in clinical psychology. Integrative literature reviews and summary reports of innovative ongoing clinical research programs are also sometimes published. Reports on individual research studies are not appropriate.

#### AUDIENCE

Psychologists and Clinicians in Psychopathy

#### IMPACT FACTOR

2009: 4.901 © Thomson Reuters Journal Citation Reports 2010

#### ABSTRACTING AND INDEXING

BIOSIS  
Behavioral Medicine Abstracts  
Current Contents/Social & Behavioral Sciences  
EMBASE  
PsycINFO Psychological Abstracts  
PsycLIT  
Psycan CP  
Research Alert  
Scopus  
Social Sciences Citation Index  
Social and Behavioural Sciences

A full copy of these guidelines can be accessed at:  
[http://www.elsevier.com/wps/find/journaldescription.cws\\_home/652/authorinstructions](http://www.elsevier.com/wps/find/journaldescription.cws_home/652/authorinstructions)

## **Appendix 2**

### **Author guidelines for submission to British Journal of Clinical Psychology**

The British Journal of Clinical Psychology publishes original contributions to scientific knowledge in clinical psychology. This includes descriptive comparisons, as well as studies of the assessment, aetiology and treatment of people with a wide range of psychological problems in all age groups and settings. The level of analysis of studies ranges from biological influences on individual behaviour through to studies of psychological interventions and treatments on individuals, dyads, families and groups, to investigations of the relationships between explicitly social and psychological levels of analysis.

The following types of paper are invited:

- Papers reporting original empirical investigations
- Theoretical papers, provided that these are sufficiently related to the empirical data
- Review articles which need not be exhaustive but which should give an interpretation of the state of the research in a given field and, where appropriate, identify its clinical implications
- Brief reports and comments

#### **1. Circulation**

The circulation of the Journal is worldwide. Papers are invited and encouraged from authors throughout the world.

#### **2. Length**

Papers should normally be no more than 5000 words (excluding abstract, reference list, tables and figures), although the Editor retains discretion to publish papers beyond this length in cases where the clear and concise expression of the scientific content requires greater length.

#### **3. Submission and reviewing**

All manuscripts must be submitted via <http://www.editorialmanager.com/bjcp/>. The Journal operates a policy of anonymous peer review.

#### **4. Manuscript requirements**

- Contributions must be typed in double spacing with wide margins. All sheets must be numbered.

- Manuscripts should be preceded by a title page which includes a full list of authors and their affiliations, as well as the corresponding author's contact details. A template can be downloaded from [here](#).
- Tables should be typed in double spacing, each on a separate page with a self-explanatory title. Tables should be comprehensible without reference to the text. They should be placed at the end of the manuscript with their approximate locations indicated in the text.
- Figures can be included at the end of the document or attached as separate files, carefully labelled in initial capital/lower case lettering with symbols in a form consistent with text use. Unnecessary background patterns, lines and shading should be avoided. Captions should be listed on a separate sheet. The resolution of digital images must be at least 300 dpi.
- For articles containing original scientific research, a structured abstract of up to 250 words should be included with the headings: Objectives, Design, Methods, Results, Conclusions. Review articles should use these headings: Purpose, Methods, Results, Conclusions.
- For reference citations, please use APA style. Particular care should be taken to ensure that references are accurate and complete. Give all journal titles in full.
- SI units must be used for all measurements, rounded off to practical values if appropriate, with the imperial equivalent in parentheses.
- In normal circumstances, effect size should be incorporated.
- Authors are requested to avoid the use of sexist language.
- Authors are responsible for acquiring written permission to publish lengthy quotations, illustrations, etc. for which they do not own copyright. For guidelines on editorial style, please consult the [APA Publication Manual](#) published by the American Psychological Association.

## 5. Brief reports and comments

These allow publication of research studies and theoretical, critical or review comments with an essential contribution to make. They should be limited to 2000 words, including references. The abstract should not exceed 120 words and should be structured under these headings: Objective, Method, Results, Conclusions. There should be no more than one table or figure, which should only be included if it conveys information more efficiently than the text. Title, author name and address are not included in the word limit.

## 6. Supporting Information

BJC is happy to accept articles with supporting information supplied for online only publication. This may include appendices, supplementary figures, sound files, videoclips etc. These will be posted on Wiley Online Library with the article. The print version will have a note indicating that extra material is available online. Please indicate clearly on submission which material is for online only publication. Please note that extra online only material is published as supplied by the author in the same file format and is not copyedited or typeset. Further information about this service can be found at <http://authorservices.wiley.com/bauthor/suppmat.asp>

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## 8. Colour illustrations

Colour illustrations can be accepted for publication online. These would be reproduced in greyscale in the print version. If authors would like these figures to be reproduced in colour in print at their expense they should request this by completing a Colour Work Agreement form upon acceptance of the paper. A copy of the Colour Work Agreement form can be downloaded [here](#).

## 9. Pre-submission English-language editing

Authors for whom English is a second language may choose to have their manuscript professionally edited before submission to improve the English. A list of independent suppliers of editing services can be found at [http://authorservices.wiley.com/bauthor/english\\_language.asp](http://authorservices.wiley.com/bauthor/english_language.asp). All services are paid for and arranged by the author, and use of one of these services does not guarantee acceptance or preference for publication.

## 10. Author Services

Author Services enables authors to track their article - once it has been accepted - through the production process to publication online and in print. Authors can check the status of their articles online and choose to receive automated e-mails at key stages of production. The author will receive an e-mail with a unique link that enables them to register and have their article automatically added to the system. Please ensure that a complete e-mail address is provided when submitting the manuscript. Visit <http://authorservices.wiley.com/bauthor/> for more details on online production tracking and for a wealth of resources including FAQs and tips on article preparation, submission and more.

## 11. The Later Stages

The corresponding author will receive an email alert containing a link to a web site. A working e-mail address must therefore be provided for the corresponding author. The proof can be downloaded as a PDF (portable document format) file from this site. Acrobat Reader will be required in order to read this file. This software can be downloaded (free of charge) from the following web site:

<http://www.adobe.com/products/acrobat/readstep2.html>.

This will enable the file to be opened, read on screen and annotated direct in the PDF. Corrections can also be supplied by hard copy if preferred. Further instructions will be sent with the proof. Hard copy proofs will be posted if no e-mail address is available. Excessive changes made by the author in the proofs, excluding typesetting errors, will be charged separately.

## 12. Early View

British Journal of Clinical Psychology is covered by the Early View service on Wiley Online Library. Early View articles are complete full-text articles published online in advance of their publication in a printed issue. Articles are therefore available as soon as they are ready, rather than having to wait for the next scheduled print issue. Early View articles are complete and final. They have been fully reviewed, revised and edited for publication, and the authors' final corrections have been incorporated. Because they are in final form, no changes can be made after online publication. The nature of Early View articles means that they do not yet have volume, issue or page numbers, so they cannot be cited in the traditional way. They are cited using their Digital Object Identifier (DOI) with no volume and issue or pagination information. E.g., Jones, A.B. (2010). Human rights Issues. *Human Rights Journal*. Advance online publication. doi:10.1111/j.1467-9299.2010.00300.x

## Appendix 3

### Details of database search



Friday, May 27, 2011 5:24:56 AM

#	Query	Limiters/Expanders	Last Run Via	Results
S10	S8 or S9	Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Advanced Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	528
S9	Child Acceptance and Mindfulness Measure OR Cognitive and Affective Mindfulness Scale OR Five Facet Mindfulness Questionnaire OR Freiburg Mindfulness Inventory OR Kentucky Inventory of Mindfulness Skills OR Langer Mindfulness/Mindlessness Scale OR Mindful Attention Awareness Scale OR Mindfulness-Based Relapse Prevention Adherence and Competence Scale OR Mindful Coping Scale OR Mindfulness Questionnaire OR Philadelphia Mindfulness Scale OR Southampton Mindfulness Questionnaire OR Toronto Mindfulness Scale	Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Advanced Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	131
S8	S1 and S7	Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Advanced Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	386
S7	SU measure* OR questionnaire OR scale OR instrument	Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Advanced Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full	426813

			Text;PsycINFO	
S6	S1 and S2	Limiters - Published Date from: 19500101-20110631; English Language; English Language; Human; English Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Basic Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	2836
S5	S1 and S2	Limiters - English Language; English Language; English Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Basic Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	2942
S4	S1 and S2	Narrow by Subject - Scales Narrow by Subject - Questionnaires Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Basic Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	429
S3	S1 and S2	Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Basic Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	3029
S2	measure* OR questionnaire OR scale OR instrument	Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Basic Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	3326014
S1	mindful* OR meditat*	Search modes - Boolean/Phrase	Interface - EBSCOhost Search Screen - Basic Search Database - Psychology and Behavioral Sciences Collection;CINAHL Plus with Full Text;MEDLINE with Full Text;PsycINFO	14620

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## Appendix 4

### Excluded studies from systematic review

Study	Name of scale	Acronym	Rationale for exclusion
Bergomi et al. (2011)	Comprehensive Inventory of Mindful Experiences	CHIME	Not published in English
Brown & Ryan (2004)	-	-	Review article
Brown et al. (2011)	Mindful Attention and Awareness Scale - Adolescent	MAAS-A	Non-adult population
Cardaciotto (2005)	-	-	Unpublished thesis later published in Cardaciotto et al. (2008)
Chawla et al. (2010)	Mindfulness-Based Relapse Prevention Adherence and Competence Scale	MBRP-AC	Measure of treatment fidelity not individual mindfulness
Christopher et al. (2009)	-	-	Validating several scales with Thai Buddhist monks
Drake (2007)	Implicit Relational Assessment Procedure	IRAP	Behavioral measure not self report
Duncan (2007)	Interpersonal Mindfulness in Parenting Scale	IEM-P	Measures inter-personal mindfulness not individual
Fresco et al. (2007)	Experiences Questionnaire	EQ	Does not measure attention
Govern & Marsch (2001)	Situational Self-Awareness Scale	SSAS	Does not measure acceptance
Greco et al. (2008)	Avoidance and Fusion Questionnaire for Youth	AFQ-Y	Non-adult population
Greco et al. (2011)	Child and Adolescent Mindfulness Measure	CAMM	Non-adult population
Hayes et al. (2004)	Acceptance and Action Questionnaire	AAQ	Does not measure attention
Höfling et al. (2011a)	Mindful Attention and Awareness Scale	MAAS	Questionnaire not English version (German)
Höfling et al. (2011b)	-	-	Validation of scale (KIMS) not in English
Johnson (2007)	Mindful Attention and Awareness Scale	MAAS	Questionnaire not in English (Spanish)
Kohls et al. (2009)	-	-	Validation of scale (FMI) not in English
Kraus & Sears (2008)	Self-Other Four Immeasurables Scale	SOFI	Does not measure attention
Mascaro et al. (2004)	Spiritual Meaning Scale	SMS	Does not measure either aspect of mindfulness



Study	Name of scale	Acronym	Rationale for exclusion
McCracken et al. (2010)	Chronic Pain Acceptance Questionnaire	CPAQ	Does not measure attention
Reavley & Pallant (2009)	Effects of Meditation Scale	EOM	Does not measure attention
Sherman (2006)	Langer Mindfulness Scale – Youth Version	LMS-Y	Unable to access full text and non-adult population
Solloway & Fisher (2007)	Solloway Mindfulness Survey	SMS	Measure of how mindful an individual is during mindfulness practice rather than individual mindfulness <i>per se</i>
Tharaldsen & Bru (2011)	Mindful Coping Scale	MCS	Questionnaire not in English (Norwegian)
Walach et al. (2006)	-	-	Validation of scale (FMI) not in English
West (2008)	Mindful Thinking and Action Scale for Adolescents	MTASA	Unpublished measure and unable to access full text. Non-adult population.

## Appendix 5

Table of quality of life outcome measures

Measure/ domain	Overall quality of life	Physical functioning	Physical health	Social functioning	Mental/ emotional health
EORTC- QLQ	✓	✓	✓	✓	✓
FACT-G		✓	✓	✓	✓
FAHI	✓		✓	✓	✓
FIQ	✓	✓	✓		✓
FIS		✓		✓	✓
HAQUAMS (German)		✓		✓	✓
IBS-QOL		✓		✓	✓
QLI*	✓	✓	✓	✓	✓
QOL (German)	✓	✓		✓	✓
SF-12		✓	✓	✓	✓
SF-36		✓	✓	✓	✓
Veterans SF- 36		✓	✓	✓	✓

A tick indicates the measure assesses the particular domain

EORTC QLQ-C30: European Organisation for Research and Treatment of Cancer Quality of Life Questionnaire; FACT-G: Functional Assessment of Chronic Illness Therapy-General; FAHI: Functional Assessment of HIV Infection; FIQ: Fibromyalgia Impact Questionnaire; FIS: Fatigue Impact Scale; HAQUAMS: Hamburg Quality of Life Questionnaire in Multiple Sclerosis; IBS-QOL: Irritable Bowel Syndrome –Quality of Life Measure; QLI: Quality of Life Index; QOL: Profile of Health-Related Quality of Life in Chronic Disorders; SF-12: Short Form-12 Health Survey; SF-36: Short Form-36 Health Survey; Veterans SF-36: Veterans Short Form-36

## Appendix 6

Mathematical formula for calculation of effect size

**Cohen's  $d$ :**

$d = \frac{M_1 - M_2}{SD}$ , where  $M_1$  and  $M_2$  are the mean scores of the two conditions being compared.

**Hedges  $g$ :**

$g = d \times J$  where  $J = 1 - \frac{3}{4df - 1}$ . Here,  $df$  is the degrees of freedom and is calculated as:  $df = n_1 + n_2 - 2$ .

**Within group effect size (repeated measures):**

$d_{RM} = \frac{M_{post} - M_{pre}}{SD_{difference}} \times \sqrt{2(1-r)}$ , where  $r$  is the correlation between the pre and post test scores (Morris & DeShon, 2002, p.108; Borenstein *et al.*, 2009) and where,

$$SD_{difference} = \sqrt{SD_{post}^2 + SD_{pre}^2 - 2 \times r \times SD_{post} \times SD_{pre}}$$

For within group effect sizes, the variance of Cohen's  $d$  was estimated using:

$$V_d = \left( \frac{1}{n} + \frac{d^2}{2n} \right) 2(1-r)$$

This was converted to Hedge's  $g$  by:

$$V_g = J^2 \times V_d$$

The standard error of Hedge's  $g$  was estimated by:

$$SE_g = \sqrt{V_g}$$

**Between group effect size:**

$$d_{IG} = \frac{(M_{post,MBSR} - M_{pre,MBSR})}{SD_{pre,MBSR}} - \frac{(M_{post,control} - M_{pre,control})}{SD_{pre,control}}$$

For between group effect sizes, the variance was estimated using:

$$V_d = \frac{n_1 + n_2}{n_1 n_2} + \frac{d^2}{2(n_1 + n_2)}, \text{ where } n_1 \text{ and } n_2 \text{ are the sample size of each group and } d \text{ is}$$

the between group effect size. This was converted to the standard error of Hedge's  $g$  as above (Borenstein *et al.*, 2009).

***Fail-safe N (Rosenthal, 1979)***

$$N_{FS} = \frac{\left(\sum_{i=1}^k z_i\right)^2}{2.706} - k, \text{ where } z \text{ is the z-score of each effect size and } k \text{ is the number of}$$

studies in the meta-analysis (Field & Gillett, 2010). Rosenthal considers the findings robust if the required number of studies ( $N_{FS}$ ) to reduce the overall effect size to a nonsignificant level exceeds  $5k+10$ .

## Appendix 7

### Excluded studies from meta-analysis

Study*	Rationale for exclusion
Ando <i>et al.</i> (2009)	No QoL measure
Birnie <i>et al.</i> (2010)	No QoL measure
Branstrom <i>et al.</i> (2010)	No QoL measure
Carlson <i>et al.</i> (2001)	No QoL measure
Carlson <i>et al.</i> (2004)	Same data as described in Carlson <i>et al.</i> (2003)
Carlson <i>et al.</i> (2005)	No QoL measure
Cho <i>et al.</i> (2010)	No MBSR intervention
Creswell <i>et al.</i> (2009)	No QoL measure
Curiati <i>et al.</i> (2005)	Not an MBSR intervention
De Vibe & Moum (2006)	Not in English; not solely chronic health population
Downey <i>et al.</i> (2009)	No QoL measure
Elsenbruch <i>et al.</i> (2005)	Not MBSR
Fang <i>et al.</i> (2010)	Heterogeneous population – not solely chronic health population
Faude-Lang <i>et al.</i> (2010)	In German
Flugel-Colle <i>et al.</i> (2010)	Not solely chronic health population
Foley <i>et al.</i> (2010)	Intervention MBCT not MBSR
Garland <i>et al.</i> 2007	No QoL measure
Goldenberg <i>et al.</i> (1994)	Not enough data reported
Goldenhar (2005)	Couldn't access abstract or full text
Gross <i>et al.</i> (2009)	Study rationale only - not empirical study
Gross <i>et al.</i> (2011)	Not chronic health population (includes chronic insomnia)
Jacobs (2002)	Not chronic health population
Jam <i>et al.</i> (2009)	No QoL
Joo <i>et al.</i> (2010)	No QoL measure
Kaplan <i>et al.</i> (1993)	Not enough data to calculate effect size (i.e. no pre-post scores)
Kassardjian <i>et al.</i> (2008)	Outcome data for SF-36 not reported, validation study only
Kieviet-Stijnen <i>et al.</i> (2008)	QoL not considered adequate – one factor visual analogue scale
Kreitzer <i>et al.</i> (2005)	No QoL measure
Kvillemo & Branstrom (2011)	Qualitative study/No QoL measure used
Lush <i>et al.</i> (2009)	No QoL measure
Mackenzie <i>et al.</i> (2007)	Qualitative study/No QoL measure used
Majumdar <i>et al.</i> (2002)	Not solely chronic health population
McCain <i>et al.</i> (1996)	No MBSR intervention
McCracken & Velleman (2010)	No MBSR intervention
Michalsen <i>et al.</i> (2005)	Not exclusively MBSR – used CBT and intervention lasted a year
Moriconi (2004)	Not just chronic health pop (partners incl as well)
Morone <i>et al.</i> (2008)	Qualitative study/no QoL measure used
Nash-McFeron (2006)	Unable to access full text
Nyklíček & Kuijpers (2008)	Not chronic health population
Ostafin <i>et al.</i> (2006)	Not MBSR
Pauzano-Slamm (2005)	No QoL measure
Plews-Ogan <i>et al.</i> (2005)	Not enough data - unable to contact by email

<b>Study*</b>	<b>Rationale for exclusion</b>
Pool (1995)	Not chronic health population
Pradhan <i>et al.</i> (2007)	No QoL measure
Reibel <i>et al.</i> (2001)	Not solely chronic health population
Robert-McComb <i>et al.</i> (2004)	Only reports SF-36 physical component scores
Roth & Robbins (2004)	Not just chronic health population (mental health problems as well)
Sagula & Rice (2004)	No QoL measure
Sampali <i>et al.</i> (2009)	No QoL measure
Sephton <i>et al.</i> (2007)	Used FIQ but only as a control variable not as an outcome measure
Shapiro (2002)	Unable to access full text (dissertation)
Shapiro <i>et al.</i> (2003)	No enough data
Shigaki <i>et al.</i> (2006)	Not empirical study - review article
Sibinga <i>et al.</i> (2008)	Adolescent population
Specia <i>et al.</i> (2000)	No QoL measure
Sullivan <i>et al.</i> (2009)	No QoL measure
Tacon & McComb (2009)	Describes study rationale only – no results published yet
Tacon (2008)	Book chapter
Tacon <i>et al.</i> (2003)	No QoL measure
Tacon <i>et al.</i> (2004)	No QoL measure
Williams <i>et al.</i> (2005)	Not a MBSR intervention
Wong (2009)	No raw data reported
Young (1999)	Qualitative study/No QoL measure used
Zautra <i>et al.</i> (2008)	No QoL measure and not strictly MBSR programme

\*These studies are listed separately in the reference section

## Appendix 8

### QUOROM statement checklist

Heading	Subheading	Descriptor	Page
<b>Title</b>		Identify the report as a meta-analysis	95
<b>Abstract</b>		Use a structured format	95
		<b>Describe</b>	
	Objectives	The clinical question explicitly	95
	Data sources	The databases (i.e. list) and other information sources	95
	Review methods	The selection criteria (i.e. population, intervention, outcome, and study design); methods for validity assessment, data abstraction, and study characteristics, and quantitative data synthesis in sufficient detail to permit replication	95
	Results	Characteristics of the RCTs included and excluded; qualitative and quantitative findings (i.e. point estimates and confidence intervals); and subgroup analyses	95
	Conclusion	The main results	95
<b>Introduction</b>		<b>Describe</b> The explicit clinical problem, (biological) rationale for the intervention, and rationale for review	98
<b>Methods</b>	Searching	The information sources, in detail (e.g. databases, registers, personal files, expert informants, agencies, hand-searching), and any restrictions (years considered, publication status, language of publication)	100
	Selection	The inclusion and exclusion criteria (defining population, intervention, principal outcomes, and study design)	99
	Validity assessment	The criteria and process used (e.g. masked conditions, quality assessment, and their findings)	101
	Data abstraction	The process or processes used (e.g. completed independently, in duplicate)	101
	Study characteristics	The type of study design, participants' characteristics, details of intervention, outcome definitions, and how clinical heterogeneity was assessed	99
	Quantitative data synthesis	The principal measures of effect (e.g. relative risk), method of combining results (statistical testing and confidence intervals), handling of missing data; how statistical heterogeneity was assessed; a rationale for any a-priori sensitivity and subgroup analyses; and any assessment of publication bias	102

<b>Results</b>	Trial flow	Provide a meta-analysis profile summarising trial flow (see figure)	103
	Study characteristics	Present descriptive data for each trial (e.g. age, sample size, intervention, dose, duration, follow-up period)	106
	Quantitative data synthesis	Report agreement on the selection and validity assessment; present simple summary results (for each treatment group in each trial, for each primary outcome); present data needed to calculate effect sizes and confidence intervals in intention-to-treat analyses (e.g. 2x2 tables of counts, means and SDs, proportions)	104
<b>Discussion</b>		Summarise key findings; discuss clinical inferences based on internal and external validity; interpret the results in light of the totality of available evidence; describe potential biases in the review process (e.g. publication bias); and suggest a future research agenda	108

As cited in Moher *et al.* (1999)